Informed Empowerment*

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Abstract

We investigate behavioral mechanisms by which gender differences affect group decision-making: (1) when informed members form a majority, and (2) when informed individuals hold formal authority, such as veto power. We test our theory in gender-identity-blind environments. In our experiments, neither women or men have gendered expertise on the decision tasks. We examine *when* gender differences in information-seeking aggregate into group performance and *how* they affect decision quality on gender-neutral issues. We find that women are more willing than men to incur costs to obtain decision-relevant information. When women comprise the majority, their higher individual-level information-seeking accumulates, increasing the likelihood that the group makes optimal decisions. In groups where the majority is insufficiently informed, women with veto power exercise it more selectively and cautiously than men, thereby reducing suboptimal decision outcomes. These effects are most prominent in decision contexts in which acquiring information is neither trivial nor infeasible.

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Group decision-making is central to democratic governance, from juries and corporate boards (Feddersen and Pesendorfer 1997; Fiorina and Plott 1978) to international negotiation teams (Chaudoin, Hummel and Park 2024). Yet, despite comprising nearly half of the world's population, women are often underrepresented across group decision-making bodies (O'Brien 2015; Preece 2016). As governments, corporations, and civil society strive to expand women's presence in governance (Duell 2022; O'Brien and Piscopo 2019), questions persist about the implications of such efforts for group performance (Page 2007; Niederle, Segal and Vesterlund 2013; Ou and Pan 2021). Prior work has explored various ways gender dynamics influences group decision-making, but the findings are mixed: some studies report reduced participation for women in male-dominated groups (Karpowitz, Mendelberg and Shaker 2012), while others find backlash effects in female-majority settings (Atkeson and Rapoport 2003; Kathlene 1994), or suggest that women may wield as much, or more, influence than men even as the minority (Karpowitz et al. 2024; Anzia and Berry 2011; Kanthak and Woon 2015). These mixed results underscore a puzzle with three dimensions: (1) *do* behavioral differences between women and men aggregate into group decisions, (2) *how* do such differences shape the decision-making process, and (3) *when* do they improve or undermine group performance?

We address this puzzle by focusing on a fundamental feature of group decision-making: information aggregation. Importantly, we deliberately remove interaction and interpersonal communication from our design. In our experiments, following the voting literature, private information is aggregated through the voting mechanism itself. Individuals act on their privately acquired signals, and these signals are aggregated through collective choice rules. As a result, group performance depends on the distribution of information across individuals and how institutional rules translate individual decisions into collective outcomes. Moreover, following the seminal work on gender-blind auditions in orchestras (Goldin and Rouse 2000), in our experiments participants make decisions *without knowing* the gender composition of their group. Finally, the decision tasks of our experiments are gender-neutral such that neither women nor men hold domain-specific expertise or

face stereotyped expectations about competence. Through these designs, we control for alternative explanations documented in the literature (Karpowitz et al. 2024; Bordalo et al. 2019; Coffman, Flikkema and Shurchkov 2021) and identify two mechanisms within our behavioral framework through which gender differences in information acquisition and the use of power affect group outcomes, thereby addressing a gap that is rarely acknowledged in the literature.

We conceptualize women's role not simply as being present or holding power, but in how their distinctive information-seeking behaviors aggregate into and influence group decisions. Our findings reveal that women invest more costly effort than men to obtain and evaluate decision-relevant information. When women hold veto power, they exercise it more cautiously than men. On average, these behavioral differences translate into measurable effects in group performance: women-majority groups more often identify the optimal solutions to challenging tasks; even when women are a minority, groups where women hold veto power achieve better outcomes because that power is used selectively. However, these effects depend on context. When information cost is either trivial or exceedingly high, gender differences in information-acquisition and power use matter less for group success.

Our study advances the scholarly understanding of gender's impact on group decision-making in three ways. First, we isolate mechanisms that existing theories have not scientifically addressed. Most existing theoretical frameworks studying gendered effects rest on two conditions: (1) women and men have different policy preferences or end goals, and (2) women are more effective decision-makers on issues that specifically affect women. These conditions are reasonable in many contexts, but their scope is ultimately an empirical question. Not all decision tasks are gender-typed, and in gender-neutral settings (such as juries, hiring committees, and promotion panels) neither women nor men are inherently better-positioned decision-makers. Moreover, what constitutes a "women's issue" is challenging to define (Volden, Wiseman and Wittmer 2018), and the scholarly literature offers limited guidance on how to delineate issues as distinctly belonging to women, men,

or neither. As a result, there is little understanding of how gender composition shapes group performance on tasks where gendered expertise does not pertain. Our framework addresses this gap by focusing on a stage of decision-making prior to collective choice: costly information acquisition. It does not require restricting women's potential contribution to women's issues, nor does it require precisely defining such issues. Instead, by identifying behavioral differences between women and men in information-seeking and in the exercise of institutional authority, we provide a framework that can generalize women's impact on group outcomes beyond the domain of gender-typed tasks.

Second, we address *when* and *how* gender differences translate into differences in group performance. Prior work demonstrates that the number of women and whether women hold formal authority sometimes, but not always, affects collective outcomes. We theorize that the influence of informed empowerment is contingent on two conditions: (1) the degree to which members make decisions relatively independently, and (2) the cost of acquiring decision-relevant information. When members act primarily as delegates of external political entities, individual information-seeking behavior is unlikely to vary by gender in ways that affect group performance. By contrast, when information-seeking is neither trivially easy nor prohibitively costly, gender differences in information effort can produce meaningful variation in the distribution of knowledge across group members, which in turn shapes collective outcomes through institutional aggregation. This conditional logic provides a unified explanation for prior inconsistencies in the literature and clarifies when we should (and should not) expect gender composition to affect group performance.

Finally, we study gendered mechanisms in a context that has received comparatively little attention: groups whose members have aligned interests. Group decision-making is typically studied for two reasons: (1) to reconcile conflicting interests among members, or (2) to aggregate information among members who share a common goal (Ostrom 1990). The large majority of existing studies examining the effects of gender composition and formal authority in the contexts where the divergent preferences between men and women

are significant. Far less attention has been devoted to understanding how gendered mechanisms shape performance when members' goals are aligned. In groups such as juries, crisis response panels, and environmental oversight committees, members may differ in perspective and priorities while nonetheless sharing fundamental objectives: convicting the guilty and acquitting the innocent, or producing a more just and prosperous outcome. Our framework complements the existing literature by addressing this understudied setting, emphasizing the role of gender differences in information acquisition in producing informed representation. We also offer among the first systematic accounts of how the *informedness* of women in positions of formal authority affects the exercise of that authority and, through it, group performance—even when women constitute a minority of the decision-making body.

The remainder of the paper proceeds as follows. We first develop the conceptual framework, presenting our theory and the mechanisms through which gender differences in information-seeking affect the quality of group decision-making. We then describe the research design before turning to the experimental results. We begin with the overall patterns of gender differences in information acquisition, followed by the results of each of the three experiments in turn. We next examine the conditions under which these gender differences matter for group performance. We conclude with a discussion of the limitations of our design, the implications of our findings, and directions for future research.

Conceptual Framework

"Decisions are necessarily a function of information" (Arrow 1974, 49). Information is pivotal for individuals to assess the *state of the world*—the underlying reality and facts of the matter at hand (Feddersen and Pesendorfer 1997). For example, in a jury, the underlying state of the world is whether the defendant is guilty. In our study, *information* refers to pertinent and essential knowledge about the outcomes of actions (Fehrler and Janas 2021; Kartal and Tyran 2022; Ou and Tyson 2023), which diminishes uncertainty and enhances the *quality of group decision-making* (e.g., convicting the guilty and ac-

quitting the innocent) (Gilligan and Krehbiel 1989). Substantively, the quality of group decision-making can be understood as the likelihood of the group collectively choosing the optimal solution to the problem at hand.

In most non-trivial group decision-making, the state of the world is *ex-ante* unknown. An individual is considered *informed* of the state of the world if they have either acquired informative signals or develop the ability to infer the correct answer from various signals. On certain matters, individuals can draw upon their *priors*—personal experience, education, and professional skills. Women have been shown to be better informed on issues that they have already devoted more attention to than men (Reingold and Swers 2011; Volden, Wiseman and Wittmer 2018), especially those that disproportionately effect women. When the group decision-making tasks involve these issues, there is a higher likelihood that women have the informed answers (Huddy, Cassese and Lizotte 2008).

However, many decision tasks are gender neutral or, at least, not strictly gendered, such as determining the guilt of a defendant, setting the optimal speed limit, promotion and tenure decisions, budgeting for the national defense, increasing local school funding, and establishing ethical norms for sciences. Especially in situations where the optimal course of action is uncertain, individuals' *priors* can lead to diverse perspectives, yet they do not inherently point to the optimal solution. To decide the optimal course of action, individuals must exert effort in acquiring information to understand the state of the world (Austen-Smith and Banks 1996). For example, this may involve reviewing evidence to decide whether a suspect is innocent or guilty. Information and information acquisition are often costly. *All else equal*, individuals who are more willing to bear these costs are more likely to be informed.

Then, what impact do women have on group decision-making when gender-specific knowledge or expertise is not involved? We argue that women's presence influences group decision-making for another critical yet understudied reason: *women exert more costly effort than men to acquire and process information* regarding the decision at hand. They therefore use their power more effectively through informed voting. These behavioral

differences between women and men may be *correlated* with gender differences in risk and social preferences. Women are generally more risk-averse (Niederle 2016; Croson and Gneezy 2009; Eckel and Grossman 2002; Byrnes, Miller and Schafer 1999) and less likely than men to guess in uncertain situations (Atkeson and Rapoport 2003; Mondak and Anderson 2004). Meanwhile, previous studies suggest that women's priorities involve improving social welfare (Barnes 2016; Betz, Fortunato and O'Brien 2021), while men tend to derive greater fulfillment from the personal and power-related aspects of influence (Thomas, Herrick and Braunstein 2002; Ou 2024). Women in general are more collaborative in group-based actions (Thomas 1995; Volden, Wiseman and Wittmer 2013) whereas men favor an individualistic approach (Thomas 2016; Kenney 1996). While the existence of a gender gap in political knowledge remains debated (Jerit and Barabas 2017; Dassonneville et al. 2020), women may be more motivated to acquire information when issues of risk and social welfare are at stake. In certain contexts, for example as demonstrated in elites' behavior, women tend to rely more on acquired information when making decisions (Lazarus and Steigerwalt 2018), whereas men typically rely more heavily on their own judgment and discretion (Krupnikov et al. 2020). As noted in the following sections, we use incentivized games and a non-incentivized post-experiment survey to examine the reasons behind gender differences in information acquisition and voting.

We present a behavioral theory of *informed empowerment* that impacts the quality of group decision-making through two mechanisms:

(1) *Informed Representation*: If everyone holds the same voting power as others and informed members constitute the majority, the group will identify the optimal solution with a higher likelihood than the groups in which informed individuals are the minority. If women exert more costly effort to acquire information, they are more likely to be informed members, and their presence enhances accountable governance by leveraging the information they have acquired.

(2) *Informed Authority*: If members who acquired information hold positions of authority (e.g., procedural power such as vetoes (Bouton, Llorente-Saguer and Malherbe 2018)), especially in situations where the number of informed members is small, their groups can outperform the groups where uninformed individuals hold formal authority. If women are more likely than men to incur costs to be informed, they will use (veto) power more effectively, then when women have authority, they can

improve the quality of group decision-making.

Among others, the effects of these mechanisms depend on a critical exogenous variable: *information cost*. Even on similar topics, information cost can vary widely. For example, in a department or organization, promotion and tenure cases may be straightforward when records are clear but contentious with mixed or unconventional profiles. Similarly, in a jury context, some cases are easy to decide when the evidence is overwhelming, while others become more complex when the facts are unclear or conflicting. *All else equal*, the likelihood of being informed is determined by both how much effort one is willing to exert and the cost of obtaining relevant information. In scenarios in which the information cost is trivially low or prohibitively high, differences in individual effort for information may produce no differences in how informed the group is because the cost structure dominates group outcomes. But when information is attainable through effort, gender differences in effort for information and use of power can affect the quality of group decision-making through informed empowerment.

Research Design

To focus on testing mechanisms and control for confounders, we examine our questions in a context where all group members' interests are aligned (Feddersen and Pesendorfer 1997). To disentangle the effects of informed empowerment from other important drivers of gender differences in group decision-making—gender roles (Eagly, Woo and Diekman 2012; Mendelberg and Karpowitz 2016), selection effects (Kanthak and Woon 2015), careerism (Folke and Rickne 2016), stereotyping (O'Brien and Rickne 2016; Klar 2018), and party affiliations (Hassell and Visalvanich 2019; O'Brien 2018)—we construct settings in which individuals' decisions are not influenced by external parties, and we apply these settings in *fully anonymous* environments in which subjects make decisions *without knowing* the gender composition of their group, following the seminal work on gender-blind auditions in orchestras (Goldin and Rouse 2000).

As illustrated in Figure 1, compared to canonical designs where gender composition

and identity are visible during group process, our research design controls for the influence of gender-identity cues (Coffman 2014). Because group members make decisions *without knowing* gender composition, the mechanisms of informed empowerment operate differently from the evidence documented in existing findings. This allows us to disentangle gendered information-seeking from the "bundled" alternative explanations emphasized in prior work that shape women's contributions. The effects of informed empowerment do not diminish the significance of these factors documented in existing research. Rather, we demonstrate how and when gender differences in information acquisition can aggregate into group decisions. Studying bundled effects both comprehensively and in isolation is crucial for understanding the dynamics at play and identifying the distinct contribution of each (Morton and Williams 2010). Our findings highlight the importance of examining the interplay between informational and societal effects (detailed in Section Discussion).

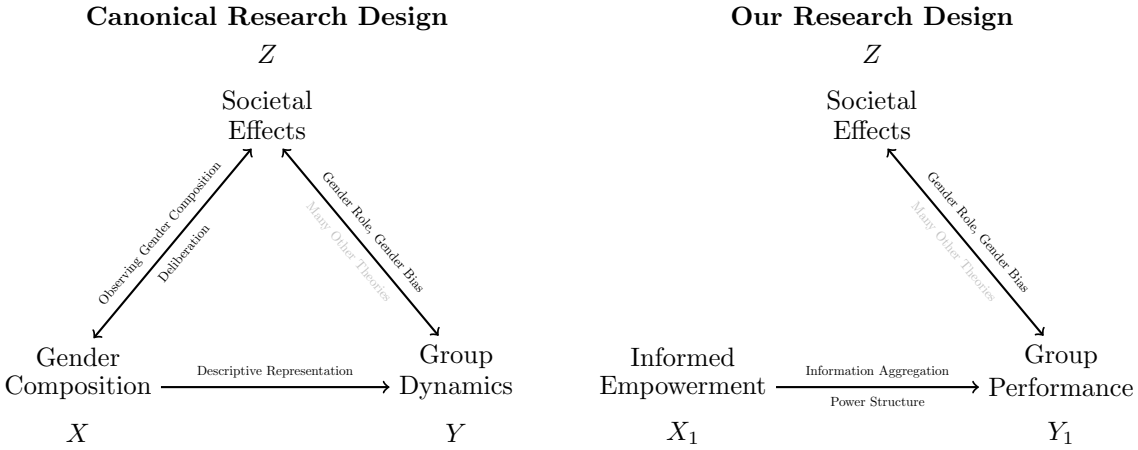


Figure 1: Isolating the Effects of Informed Empowerment. Past work often relies on a framework where the independent variable (X) is the *observed* change in gender composition, and the dependent variable (Y) is group dynamics. The mechanisms explaining how X influences Y are linked to societal effects (Z), including gender norms, stereotyping, gender bias, domain-expertise, and many other latent effects. In other words, *knowing* gender composition influences expectations and behavior. In our research design, we "erase the link" between information acquisition and voting and the perceived variation in gender composition that may shape expectations and norms. We acknowledge and recognize alternative theories and explanations (technically, the link between Z and the quality of group decision-making Y_1). Our research design allows us to isolate the effects of informed empowerment (X_1) from the "bundled" alternative explanations.

In naturally occurring situations, the counterfactual is challenging to observe, and the causal relationship between information acquisition and group outcome is difficult to identify. We thus conduct laboratory experiments to bypass these methodological hurdles

and to establish appropriate controls and measurements (Falk and Heckman 2009; Morton and Williams 2010). We used an induced value approach (Smith 1982), designed decision tasks with monetary payoffs, and assigned explicit monetary values to outcomes, ensuring that a self-interested participant would seek to maximize their earnings. Earnings were measured in experimental currency units (ECUs), which were later converted into cash (500 ECUs = \$10). This methodology has unique advantages in controlling for preferences and establishing an "all-else-equal" environment (Palfrey 2013).

We used a between-subject design and conducted three preregistered experiments using the same protocols.[1] Figure 2 illustrates the procedure applied in all of our experiments. In each session, five participants remotely joined the experiment over an application in which everyone's identity was purposely blinded. Every participant was randomly assigned an anonymous experimental ID. Participants did not know the gender composition of their group. In each experimental session, we measure collaboration preferences in Part 1 and risk preferences in Part 3 as independent components of each session (see Figure 2). Part 2, which consists of the voting games, addresses the central focus of our research questions. Across experiments, treatment variation was introduced through the exogenously assigned voting rule (Step 4 of Figure 2). Participants were randomly assigned to experimental conditions. Once participants were assigned to a group and a voting rule, these assignments remained fixed for the duration of the session.

In Part 2, the group decision-making phase, participants were organized into groups of five and asked to vote between *Jar 1* and *Jar 2* (see Supplementary Information E.3). It was common knowledge that the computer randomly selected one jar as the *state of the world* with equal probability (50%). The group's task was to correctly identify the selected jar (e.g., convicting a guilty defendant or acquitting an innocent one), which yielded a high payoff (100 ECUs); failure to do so resulted in a low payoff (0 ECUs). The resulting payoff was applied equally to all group members, regardless of individual votes.

The *state of the world* (which *Jar* is selected by the computer) is ex-ante *unknown*,

[1] Our study and hypotheses were preregistered at [link redacted]. A Pre-Analysis Plan, sample size, and power analysis are reported in Supplementary Information E.5.

but group members have the option to incur costs to acquire information about it.[2] Each participant was endowed with 100 ECUs. The computer randomly generated individual-specific information costs between 1 and 100 ECUs. The more a member is willing to pay to acquire information, the more likely they are informed. Subjects make decisions *without knowing* the gender composition. Abstaining is not allowed. Voting is simultaneous and costless.

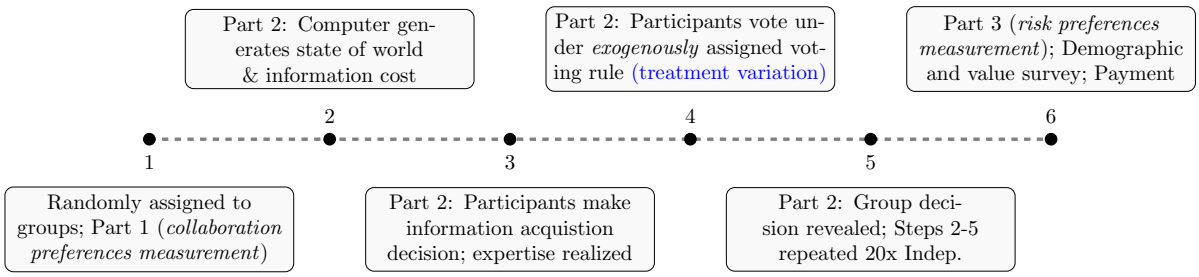


Figure 2: Timeline and Procedures of Experiment. Each paid session consists of three parts. Part 1 measures individuals' collaboration preferences through the threshold public goods game. Part 2 is the voting game that operates independently and repeatedly for 20 periods. Within the voting part, each period operates independently with varying parameterizations; the state of the world and information cost are randomly redrawn from period to period. Participants received feedback on the previous period's group outcome but no feedback regarding the realized information cost, who acquired information, who obtained expertise, or the voting decisions of others. Part 3 measures individuals' risk preferences. Instructions for Part 2 were given after Part 1, and those for Part 3 were provided at the end of Part 2. In each session, everyone received the same instructions. There was no feedback of each part's results and earnings until the end of the session. The limited feedback between periods and experiment parts cuts down on possible cross-decision influences (Fréchette 2012) and ensures that Experiment I's results are comparable to results in Experiments II and III. Participants were allowed to ask question at any time. Post-experiment survey questions are not incentivized.

We take steps to capture a set of potential effects generated through various informa-

[2]To assess the level of effort exerted by a group member, we consolidate various information gathering activities into a single measurement. This design is driven by two main considerations. First, information acquisition takes place through a variety of formal and informal channels, and individual members gather information both individually and collectively before, during, and after committee meetings. For example, jurors are not allowed to engage in discussions with attorneys, witnesses, fellow jurors, or anyone involved in the case while the trial is ongoing. They are required to focus on the evidence presented in the courtroom and the judge's instructions on the applicable law. After all evidence has been presented, jurors may share personal insights, reevaluate evidence, and seek clarification from fellow jurors before reaching a verdict. Second, because the means by which information is obtained and the cost of obtaining it varies across contexts (e.g. juries, promotion and tenure committees, corporate boards, and so on), we take a context-neutral approach. We abstract diverse forms of information gathering activities as an overall effort for information, which enables us to examine our theory and explore the research questions without concern for the application of a precise context and definition of institutional factors (Brutger et al. 2022; Morton and Williams 2010; Smith 1982). By treating information acquisition as a unified process throughout the committee proceedings, we simplify our experimental procedure, enable a more straightforward measurement of effort for information expertise, and avoid the complexities that would arise from separately measuring information acquisition.

tion gathering activities. In our experiments, when informed, this information is true 90% of the time and wrong 10% of the time. Our design suggests that information expertise helps individuals to understand the state of the world with sufficiently high confidence. It also accommodates the critical functions of information exchange before and during meetings, because all the essential knowledge one gains from meetings and communication pertains to the state of the world (Fehrler and Janas 2021; Kartal and Tyran 2022; Ou and Tyson 2023). We capture the non-essential knowledge content introduced by social context and social cues through our "noisy" signal of the state of the world (i.e., being informed wrongly 10% of the time).

Additionally, we implement necessary controls to ensure clean inference. First, our neutral group-based task guarantees that neither men or women have gendered advantages in the experiments. We also make each state of the world equally likely so that any behavioral differences identified in our work are not driven by gendered differences in misperceptions about probabilities (Kahneman and Tversky 1979). Thus, gendered expertise, gender differences in policy preferences, and selection effects are controlled.

To reduce the likelihood of Type II error caused by stochastic factors (such as lucky guesses or extremely high/low information cost stochastically generated in the experiments), participants engaged in 20 periods of voting.[3] Each period operates independently with varying parameterizations; relevant parameters—such as the state of the world and information costs—are randomly and independently redrawn from period to period. We follow the methodological advice of Fréchette (2012)—participants received feedback on the previous period's voting outcome but no feedback regarding the realized information cost, who acquired information, who obtained expertise, or the voting decisions of others. The limited feedback between periods and experiment parts cuts down on possible cross-decision influences.

[3]Our findings are robust to these stochastic factors, see Supplementary Information C.

Overall Gender Differences in Information Acquisition

The experiments were conducted at a standard experimental social science laboratory at [university name redacted]. The average duration of a session is about 75 minutes. The average earnings of each participant were about $23, including a $7 show-up payment. A total of 220 undergraduate student subjects participated in our study. Individuals' willingness to acquire information is measured on a scale from 0 to 100 ECUs. We find that on average men are willing to pay 23.53 ECUs to acquire information. Women are willing to pay 58.73, about 2.5 times the average amount men pay ($\Delta = 35.20$, $n = 220$, $p < 0.001$.) When we break down the analysis by experiment, detailed in Supplementary Information B.4, the results are similar.

We find no difference in cognitive ability between women and men participants, measured by a cognitive reflection task (Frederick 2005) implemented post main experiment. We also find both men and women in general follow their acquired information to vote when informed. When uninformed, they are equally likely to vote for *Jar 1* or *Jar 2*, with a probability of 50%. There is no significant difference in this behavioral pattern across experiments.

However, social and risk preferences are significantly correlated with individuals' information acquisition. We used a *threshold public good game* to measure collaborative preferences. Each member of the 5-person group decides how much of 100 ECUs to contribute to a group account. Only when the group's aggregate contribution is equal to or over the threshold (i.e., 250 ECUs), individuals can benefit from the group work. Otherwise, if the group's aggregate contribution is less than the threshold, everyone loses their contribution. We used individuals' contributions to the group's account as a proxy to examine whether women are more collaborative (Palfrey and Rosenthal 1984; Bicchieri 2016). We also measure risk preference by asking participants how much (between 0 and 100 ECUs) they want to invest in a risky asset, which has a 25% chance of tripling the investment and a 75% chance of losing it. The rest is kept safe (Gneezy and Potters

1997). Both the measurements of collaborative and risk preferences are incentivized with real monetary consequences.[4]

We find women contribute 65.86 ECUs to the group account in the threshold public good game, which is significantly more than the 47.85 ECUs contributed by men ($n = 220$, $t = 6.414$, $p < 0.001$). Meanwhile, women invest 31.75 ECUs to a risky option, which is significantly less than the 50.85 ECUs invested by men ($n = 220$, $t = 4.453$, $p < 0.001$). We further find that individuals' tendencies to contribute in collective action is positively correlated with their effort for expertise, and their risk preference is negatively correlated with their effort for information (Figure 3 and Supplementary Information B.6, B.7). The correlations are not surprising, as information acquisition (undertaken at an individual cost for the benefit of the group) constitutes a form of public good provision.

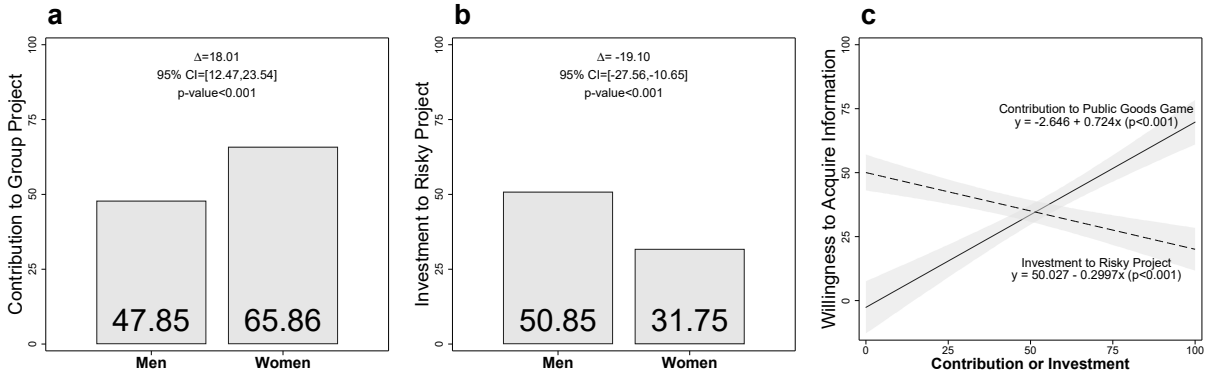


Figure 3: Gender Differences in Collaboration and Risk Preferences. **(a)** Gender differences in contributing to the threshold public goods game. **(b)** Gender differences in investments to the risky project. **(c)** Correlations between the effort for information and collaboration and risk preferences. *Takeaway*: Women exert more costly effort than men to acquire information about the *state of the world*, correlated with women's greater willingness to contribute and higher risk aversion. All measurements are based on incentivized games.

In addition to the incentivized tasks and measurements described above, participants completed a short, unincentivized post-experiment survey before the results from the three parts of the experiment were revealed. Specifically, we asked (1) "How important was it to you to acquire information?" (2) "When you make information acquisition and voting decisions, to what extent you think about your own benefits and to what extent you think about the group's total benefits?" (3) "To what extent do you think your vote

[4]To avoid the influence of playing previous part(s) on the decisions in latter part(s), participants received no feedback regarding the outcomes of each part until the end of the session.

affects the voting outcomes?"' As detailed in Supplementary Information B.3, gender differences in information acquisition based on incentivized games are also supported by the results of our unincerntivized post-experiment survey that it was more important for women to become informed because they care more about the group's aggregate benefits.

Experiment I: Informed Representation

In Experiment I, we examine two experimental conditions: **4M1W** (each group consists of 4 men and 1 woman) and **2M3W** (each group consists of 2 men and 3 women). Eighty subjects participated in Experiment I. Participants are randomly assigned to experimental conditions. Once participants were assigned to a group, it was fixed with no rematching. Subjects make decisions *without knowing* the gender composition of their group. In Experiment I, on average men are willing to pay 20.61 to acquire information. Women are willing to pay 50.18, about 2.5 times the average amount men pay ($\Delta = 29.58$, $p < 0.001$).[5] As a result, on average women obtained expertise about 50% of the time, which is significantly higher than the 21% likelihood that men obtained expertise.

The gender difference in information acquisition results in different levels of information expertise between experimental conditions. In **4M1W**, the average proportion of group members who are informed of the *state of the world* is about 20%, but in **2M3W**, it is significantly higher at 45%.[6] We measure the quality of group decision-making by examining the likelihood groups' collective choices match the *state of the world*. We find that the quality of group decision-making is 61% in **4M1W**, but 80% in **2M3W**, a 19-percentage point difference and 31% increase in decision accuracy, a difference that is both statistically and substantively significant (Figure 4, Column 1 of Table 1).[7]

[5]We present a benchmark pivotal voting model for our experimental games in Supplementary Information A. We solve symmetric equilibrium in which voters with the same information and payoff trade-offs follow identical equilibrium strategies. Men's average information acquisition effort is close to the symmetric equilibrium; women's effort is higher. We explore these further in Section Discussion.

[6]In a less controlled environment, the comparisons between **4M1W** and **2M3W** should consider the behavioral effects that we controlled, such as women are more likely to engage in discussion and make their voice heard when they observe a majority of women on the committee. We believe those other behavioral effects will further amplify the effects of *Informed Representation* as the changed dynamics among committee members may decrease the cost of information acquisition.

[7]We use a regression-based statistical analysis and use cluster-robust standard errors, allowing for

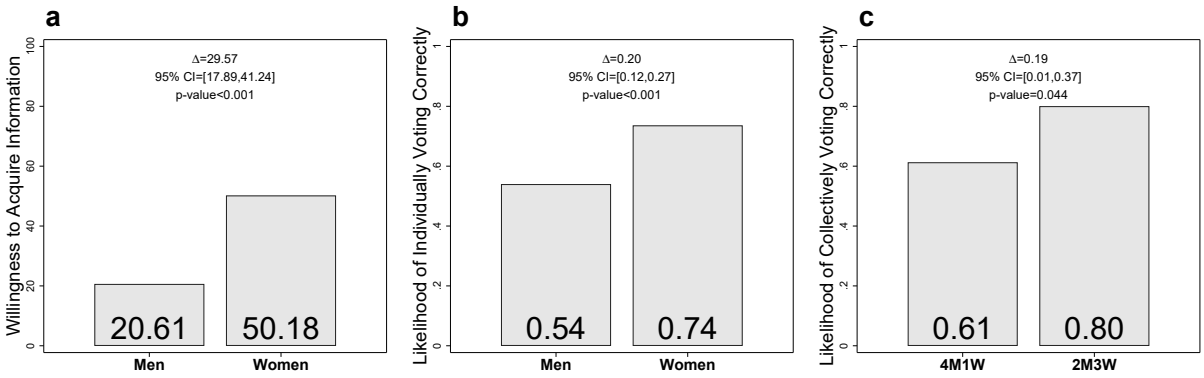


Figure 4: Gender Differences and Effects of Informed Representation (Experiment I). (a) The willingness to acquire information. **(b)** The likelihood of individually voting for the state of the world. **(c)** The groups' likelihood of voting for the state of the world. *Takeaway*: Since women on average exert more effort for information, they are more likely to be informed and thus vote correctly, and a group in which women are the majority is more likely to identify the state of the world than a group in which women are the minority.

Experiment II: Informed Authority

In Experiment II, we examine the effects of *Informed Authority* under a given gender composition in which women are the minority. Specifically, we study two experimental conditions in which each group consists of 4 men and 1 woman: **MVeto-4M1W** (a man is the veto player) and **WVeto-4M1W** (a woman is the veto player). Thus, our empirical benchmark is based on the results from the **4M1W** condition. Another eighty subjects participated in Experiment II.

Why should we study *Informed Authority* conditional on **4M1W**? Our design choice is based on three reasons. First, Experiment II is designed to isolate the effects of *Informed Authority* from otherwise "bundled" effects. If we do not control for gender composition and instead introduce conditions in which women are the majority and men are the minority, the effects of *Informed Representation* would confound those of *Informed Authority*. Second, as discussed in Section Research Design and illustrated in Figure 1, our fully anonymous experimental environment allows us to identify informed empowerment while ensuring that individual group members' power and influence are only determined by exogenous institutional arrangements but not endogenously affected by whether (they

correlation between observations within a given committee. We also examine our findings by performing an arguably more conservative, analogous non-parametric analysis (reported in Supplementary Information B.1). The two statistical analyses generate qualitatively identical results.

see) they are in the minority or majority. This feature distinguishes our theoretical framework from the existing gender literature in which seeing being a majority member is more influential and powerful. Finally, given that participants make decisions *without knowing* the group's gender composition or whether a man or a woman holds veto power, variation in experimental conditions should not affect their latent pivotality calculations, perceived influence, or strategic behavior (which we further examine in Experiment III).[8]

We use veto in our experiment to represent formal power and authority for two reasons. First, this design enables us to precisely investigate how men and women, especially those who have formal authority, use their power differently, and how their level of informedness affects their decisions and group outcomes. Second, our design is one of the more straightforward ways to convey to participants the procedural power and formal leadership of authority figures. Veto power is one of the most representative modes of formal authority (Gilligan and Krehbiel 1987; Tsebelis 1995; Bouton, Llorente-Saguer and Malherbe 2018) and vetoes are common across a variety of decision-making bodies (Hassell and Kernell 2016). The American House and Senate each have veto power over a bill after it is sent back from a conference committee; Provost or Deans at universities may have veto power over promotion and tenure decisions; and the chair of the board and CEOs also wield veto power over crucial decisions in corporate governance.

We heretofore refer to the members with veto power as the *veto players*. To make a decision, the group first performs the voting procedure that is identical to *majority voting* implemented in Experiment I. The veto player makes information acquisition and voting decisions like the other members. After everyone casts the vote, the veto player is told how many votes are for *Jar 1* and how many votes are for *Jar 2*. Then the veto player decides whether to veto. We set *Jar 2* as the default winning option (i.e., status-quo). When the veto is not used, the voting outcome is the majority's choice. The veto player can block changes from the default winning option. When the veto is used, even if the

[8]Our design can be viewed as an arguably "least likely" test for detecting gendered effects on group outcomes. Our estimates may therefore represent *lower bounds* of the effects of informed empowerment. We further explore in Section Implications that the relationship between *Informed Representation* and *Informed Authority* may be nonlinear.

majority votes for *Jar 1*, the veto player can block passing *Jar 1* and thus make *Jar 2* the voting outcome.[9] In our experiments, only veto players knew whether a veto was used; the regular group members received no feedback regarding how the others voted and were not told whether a veto was used.[10]

A rational veto player should not use their veto when they are uninformed, but should follow the choice of the majority (Feddersen and Pesendorfer 1996; Battaglini, Morton and Palfrey 2010; Morton and Tyran 2011). Rational veto players will only use their vetoes when they are informed and when the majority chooses the alternative that is different from their informed state of the world. While a veto player may be a man or a woman, if women are more likely to exert costly effort to acquire information than men, then women in general should be more likely to be informed. Also, as discussed earlier, women are more inclined to utilize political power to drive policy changes, while men are documented to leverage power for personal fulfillment. Consequently, informed women in the role of veto players are well positioned to wield their power. When the majority of the group fails to identify the state of the world, veto power will play a critical role, particularly when women are veto players.

In Table 1 we report the comparisons of group performance across experimental conditions. The default comparison treatment is **4M1W**. The dependent variable is the quality of group decision-making. To identify the effects of how women and men use veto power on group outcomes, we compare the results of **MVeto-4M1W** and **WVeto-4M1W** to the results of **4M1W**, respectively. We find that the quality of group decision-making in **MVeto-4M1W** is statistically identical to in **4M1W**. However, even if the gender composition is identical and there is only one woman in each group, when women serve as

[9]Setting either *Jar 1* or *Jar 2* as the winning default option does not affect the analysis. The veto player can only block changes from the default winning option, which distinguishes critical roles from "dictator power," where the player can unilaterally decide the voting outcome. Consider a local committee, which votes to change the speed limit from 35mph to 45mph. A veto player may have private information about accidents on this road, which will increase if the speed limit is raised, and veto a change from the status quo to 45mph. They cannot, however, veto if the group decides to maintain the extant speed limit. So too in our study, the veto player can veto a change from status-quo *Jar 2* to new policy *Jar 1* but cannot veto the group's decision to maintain the status quo.

[10]This ensures that regular members' behavior is comparable between Experiment II and Experiment I and enables us to perform relevant robustness checks reported in Supplementary Information C.2.

the veto player, groups in **WVeto-4M1W** perform better than groups in **4M1W**. The groups achieve high-payoff outcomes 63% of the time in **MVeto-4M1W**, but about 87% of the time in **WVeto-4M1W**, a 24 percentage point difference and a 38% increase in decision quality. The difference is both statistically and substantively significant (Column 4 of Table 1).

	Dependent Variable: High-Payoff Outcome = 1				
Treatment Variables	(1)	(2)	(3)	(4)	(5)
2M3W ($\hat{\beta}_1$)	0.189**				0.180***
	(0.084)				(0.082)
MVeto-4M1W ($\hat{\beta}_2$)		0.019		0.017	0.016
		(0.055)		(0.049)	(0.046)
WVeto-4M1W ($\hat{\beta}_3$)			0.260***	0.281***	0.270***
			(0.065)	(0.071)	(0.069)
Observations	320	320	320	480	640
Clustered Analysis	Yes	Yes	Yes	Yes	Yes
Log-Likelihood	-186.883	-212.145	-169.020	-274.346	-354.410

Table 1: Effects on the Likelihood of Achieving High-Payoff Outcomes (Experiments I&II). Each column reports average marginal effects from Probit regressions. Robust standard errors are reported in parentheses. The independent variables *2M3W*, *MVeto-4M1W*, and *WVeto-4M1W* are binary indicators that represent voters' experimental groups. The default comparison condition is *4M1W*. Column 1 focuses on *Informed Representation*. Columns 2-4 investigate *Informed Authority*. Those results are qualitatively the same to the results reported in the Supplementary Information B.1 based on the non-parametric Fish-Pitman permutation tests. ***, **, and * denote significance at the 1%, 5%, and 10% levels.

Experiment III: Assessing Alternative Explanations

Collective decision-making may cause *strategic effects*, wherein information acquisition and voting decisions are affected by actions of others. Individuals may also strategically comport themselves to influence the decisions of others. While we impose controls on information dynamics and feedback in Experiments I and II and find no evidence that regular group members' behavior differs across these two experiments, two questions remain. First, to what extent are the observed gender differences in group decision-making driven by strategic responses to varying experimental conditions (e.g., gender composition or the allocation of veto power)? Second, to what extent do regular members and veto

players act strategically to influence others' decisions? Addressing these questions is critical for interpreting the implications of our findings.

In particular, strategic effects in Experiment II manifest in two forms. First, regular group members may strategically respond to veto players' actions, and reduce their effort to acquire information under the assumption that the veto players will make a pivotal decision. Second, veto players may strategically deploy their veto authority to influence the decision-making of other group members. For example, although the veto player is uncertain about who is informed and how much effort was exerted, the veto player may exert low effort to acquire information or engage in uninformed vetoing to "punish" other members. Experiment III is thus designed to examine whether our findings are robust to strategic effects. We used similar settings, procedures, and sixty new participants (half are women) recruited from the same subject pool to conduct Experiment III.

To isolate strategic effects, each participant was matched with four members (who did not participate in Experiment III) from a group arbitrarily retrieved from Experiment II. We used the decision choices of these from Experiment II, replicated identical variables such as information costs and the state of the world stochastically generated in Experiment II. Thus, participants of Experiment III made decisions under the same information and parameterizations as the participants of Experiment II, while their decisions only affect their own outcomes but have no consequences on the other group members' decisions and payoffs. In other words, the two strategic effects are removed in Experiment III.

Participants were randomly assigned to experimental conditions, 20% became regular group members, 80% became veto players. They were explicitly instructed about the experimental setting. No deceptive practices were employed. More details of Experiment III are reported in Supplementary Information B.2 and E.1. We find both regular group members' and veto players' behavioral patterns are statistically identical to the observations in Experiment II, suggesting that our results are robust to strategic effects.

Why do groups in **WVeto-4M1W** perform significantly better than those in **MVeto-4M1W**? To better answer this question, we combine results of Experiments II and III in

the analysis of veto players' behavior, as participants of Experiment III made decisions under the same parameterizations as the participants of Experiment II, and both regular members' and veto players' behavioral patterns are statistically identical to the observations in Experiment II.[11] We find that the performance differences are not driven by regular group members' behavior. We do not find any statistically significant difference in the effort for expertise (1) among men who have no veto power between **MVeto-4M1W** and **WVeto-4M1W**, (2) among women between **MVeto-4M1W** and **4M1W**, or (3) between men who have no veto power in **MVeto-4M1W** and men in **4M1W**. We report more details of these results in Appendices B.2 and B.4. These results are expected: because participants make decisions *without knowing* the group's gender composition or which member holds veto power, variations in experimental conditions should not affect their latent pivotality calculations, perceived influence, or strategic behavior.

However, we do find that the veto power induces veto players to exert significantly more effort to acquire information than the other same-gender participants. Men veto players are willing to pay an average of 34.17 ECUs to be informed, which is significantly higher than the 19.96 ECUs paid by the other men who cannot veto ($p = 0.024$) and the 20.61 ECUs paid by the men in treatments without the veto power ($p = 0.015$). Likewise, women veto players pay 74.09 ECUs to be informed, which is significantly higher than the 47.84 ECUs paid by the women who cannot veto ($p = 0.001$), and the 50.18 ECUs paid by the women in experimental conditions without the veto power ($p < 0.001$). Importantly, although both men and women veto players exert significantly more effort to acquire information than their same-gender group members, among veto players, women are still willing to pay significantly more than men in the information acquisition process ($\Delta = 39.93$, $p < 0.001$).

Information acquisition is correlated with individuals' values and beliefs. The added responsibility to veto players may explain why they exert more costly effort to acquire information as compared to other group members. We conducted a post-experiment survey

[11] As detailed in Supplementary Information C.2, whether we combine these data or analyze them separately will not change the conclusions.

in which we asked participants: "How important was it to you to acquire information?" and "To what extent do you think your vote affects the voting outcomes?" As reported in Supplementary Information B.3, relative to regular group members, we find that both for men and women, veto players expressed that information acquisition was significantly more important to them, and veto players have a stronger belief that their votes affect the voting outcomes.

How veto players use their veto power also directly affects group outcomes. A clear behavioral pattern observed in our experiment is that veto players are significantly more likely to exercise their veto power when the majority's choice differs from their individual vote (see Tables A8 and A9). There are significant gender differences in the behavioral patterns associated with the use of veto power. Among veto players, men on average were less likely to be informed than women; despite this, men ($N = 32$) used their veto 25% of the time, while women ($N = 32$) used veto power about 17% of the time ($\Delta = -0.089$, $p = 0.040$, Supplementary Information B.7). When uninformed, men used their veto approximately 22% of the time, while women vetoed only about 1% of the time ($p < 0.001$, Table A7). Further, uninformed men used their veto power about 17% of the time when their initial votes *matched* the majority's choice, and more than 30% of the time when their initial votes differed from the majority's choices.

If veto players were uninformed but used their veto power, they may undermine the efforts of other members who are informed. As a result, men veto players' uninformed vetoes negatively affect the potentially-positive effect of the veto power. This may explain why in Experiments II and III when men serve as veto players, the veto mechanism has no or even a negative effect on the quality of group decision-making. As illustrated in Figure 5 (c) and detailed in Supplementary Information B.1, if we estimate group outcomes based on individual members' vote before veto is used, then with the same set of participants, the quality of group decision-making is about 0.65 in **MVeto-4M1W** and 0.74 in **WVeto-4M1W** ($p > 0.1$). Thus, because women nearly always make informed vetoes, their serving as the veto players improves the quality of group decision-making

from 0.74 to 0.87 ($p < 0.05$) through *Informed Authority*.

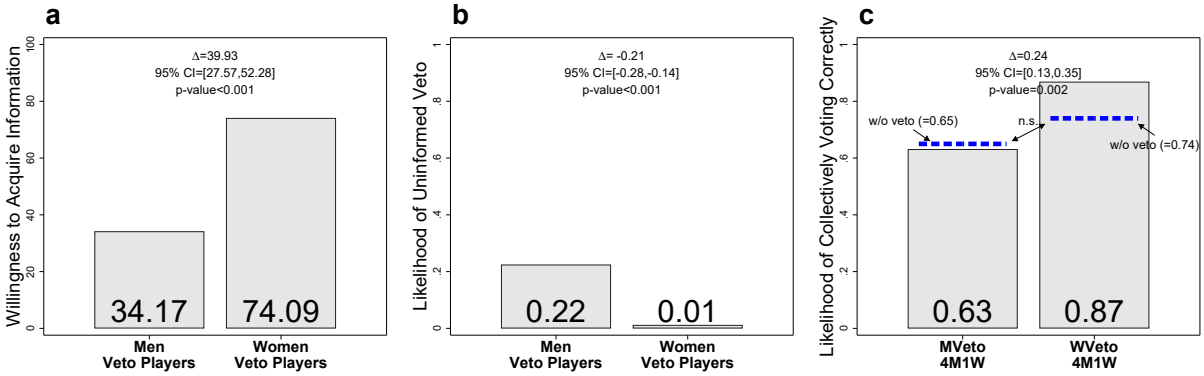


Figure 5: Gender Differences and Effects of Informed Authority (Experiments II & III). (a) Veto players' willingness to acquire information. **(b)** Veto players' likelihood of using vetoes when uninformed. **(c)** The group-level likelihood of voting for the state of the world. The blue dashed lines in (c) report the quality of group decision-making if vetoes are never used. A total of 64 participants (half are women) served as veto players. See Supplementary Information B.2 and E.1 for additional details. *Takeaway*: Women use veto with greater caution than men. When women are a minority, a group in which a woman serves as the veto player is more likely to identify the state of the world than a group in which a man serves this role.

When Gender Differences Make a Difference

We recognize that the impact of informed empowerment is conditional on various contextual factors, and we acknowledge that gender differences in information acquisition may stem from broader structural and social dynamics. For example, when individuals act as proxies for external interests, information acquisition is less important for individuals, and gender differences may not influence outcomes. Since the context of the collective decision-making decides whether gender effects are empirically significant, the lack of investigation into the distribution of gender effects across diverse group tasks may contribute to the seemingly contradictory findings regarding women's impact documented in the literature. To address this issue, the use of meta-analysis that combines results from multiple studies conducted on different samples, in different contexts, or at different times is needed (Imbens 2010; Slough and Tyson 2023).

As we report a single study's results, we could not directly investigate the effects of informed empowerment across samples, contexts, and times as a meta-analysis does. However, the comparisons between *Informed Representation* and *Informed Authority* will

help us understand alternative mechanisms that empower women to contribute to group decision-making across gender compositions and power structures. Moreover, if we abstract the variations of contexts as a function of information cost and map such variations into a distribution of information cost, then within our theoretical framework, we can leverage our experimental design and extend the investigation of gender effects beyond simple aggregate comparisons to encompass distributional analysis.[12]

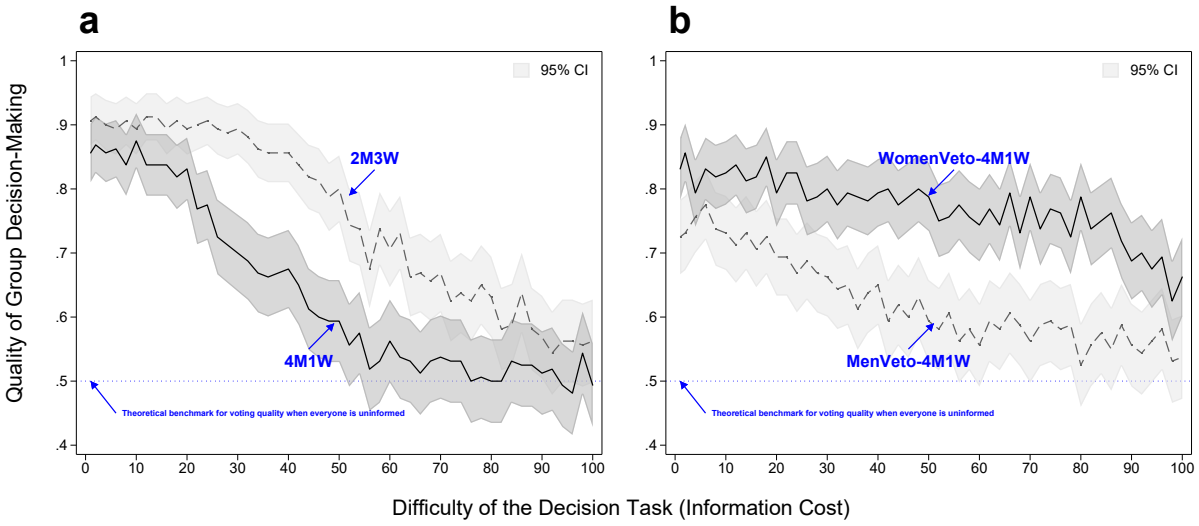


Figure 6: Distribution of Effects by Information Cost. (a) Effects of Informed Representation by information cost. **(b)** Effects of Informed Authority by information cost. Since we elicit individuals' willingness to acquire information, we can generate and assign specific information costs (from the lowest to the highest) to decision-makers post-experiment using simulations. This enables us to estimate the distribution of treatment effects in a simple and straightforward fashion, because the only difference is the realization of information cost, which was unobserved by subjects. In other words, the unobserved information cost will not change decision-makers decision rules. In estimating the distribution of effects by varying the information cost, we use a decision rule system that replicates the decision rules used by participants and follows the individual behavioral patterns observed in our experiments. We detail the protocols of the estimation in Supplementary Information D. Note that the curves of group decision-making quality are not smooth, as there is a 10% chance that informed individuals receive an incorrect signal (see Section Research Design).

Notably, *all else equal*, the average aggregate effects of both *Informed Representation* and *Informed Authority* are positive and statistically significant. Yet this aggregate perspective obscures meaningful heterogeneity, as the magnitude of these effects varies with information costs. Figures 6 illustrate the distribution of effects with respect to varying levels of information cost for *Informed Representation* and *Informed Authority*, respectively. We show that when the task is either sufficiently simple (i.e., the information

[12]Detailed methodologies are reported in Supplementary Information D.

cost is low such that everyone is informed) or complex (i.e., the information cost is high such that everyone is uninformed), gender differences in information acquisition may not yield discernible effects.[13] Moreover, when the information cost is relatively low, increasing the numbers of those who are more likely to be informed generates about the same or slightly better effects as when informed individuals hold veto power. But when the information cost is relatively high, *Informed Authority* is likely more effective than *Informed Representation*.

The information cost can vary significantly, even when the topics are similar. Whether deciding a defendant's guilt, developing an organizational strategy, allocating government budgets, making hiring or tenure decisions, responding to public health crises, or setting ethical standards in science, some cases are straightforward while others are far more complex. We speculate and as Figures 6 illustrate, informed empowerment, either through *Informed Representation* or *Informed Authority* or both, affects the group's performance in a significant proportion of cases.

Discussion

Any general theory of gender differences must be interpreted with caution. While our experimental findings provide robust support for our behavioral theory, we do not intend to make categorical claims (e.g., "men do this, women do that") or normative judgments (e.g., that one gender is more important in group decision-making than the other). Instead, we acknowledge several limitations of our study. The neutral group decision-making tasks, anonymity, and stylized procedures were designed to isolate the effects of informed empowerment from other factors characterized in Figure 1. To isolate the factors central to this study, we examine the effects of gendered information-seeking in

[13]This result should *not* be interpreted as a normative implication. Our finding is specific to the marginal effect of the two mechanisms on decision quality within our controlled experiments. When information costs are extremely low or prohibitively high, informed empowerment has limited scope to affect outcomes through the mechanisms of our behavioral theory. This does not imply that women's presence is unimportant in such settings. On the contrary, previous work shows that women's presence is critical because it can influence group dynamics along multiple dimensions specifically controlled in our study.

contexts where information acquisition is salient and members' interests are aligned. In contrast, when interests are misaligned, the true state of the world becomes ambiguous, and collective decision-making in such settings tends to reflect the aggregation of individual preferences rather than a focus on identifying the "correct" answer. Thus, our results are best suited to speak to contexts in which interests are reasonable aligned and individual actors are not proxying for other (e.g., party) interests. In our experiment, everyone's contribution is counted equally, whereas in some group decisions scenarios outside of the lab, women's contributions are often discounted due to bias. This implies that gender composition alone does not automatically improve group outcomes—potential gains may be reduced precisely *because* women's voices are undervalued. This highlights the need for field studies to test our hypotheses in less controlled settings.

Gender differences in information-seeking need *not* be intrinsic; they are likely shaped by long-standing patterns of socialization. Gender itself is a bundled treatment and we are not equipped to adjudicate whether our findings are due to biological or sociological factors. While we do find that gendered information-seeking correlates with differences in risk and collaboration preferences, we do not mean to suggest that this constitutes an comprehensive accounting of all conceivable mechanisms. We urge caution in interpreting these mechanisms, however, as our study was not designed to identify which specific (or latent) preference drives this behavior. There is little reason to assume that only one social or risk preference is relevant to the broader relationship between information-seeking and informed empowerment. The underlying mechanism matters for generalizability: if risk aversion is the main driver, the effect of informed empowerment may be muted among particular populations (such as politicians), who are generally more risk-tolerant; if collaboration preferences dominate, the effect may persist even among elites. Identifying the causal links between these preferences and gendered information-seeking is an important avenue for future research.

An important benchmark for interpreting our results is the symmetric Bayesian Nash equilibrium of the pivotal voting model presented in the Supplementary Materials. Under

this framework, individuals acquire information up to the point where the marginal cost equals the expected pivotal benefit. Empirically, men's information acquisition behavior closely tracks this equilibrium prediction, suggesting that their choices are broadly consistent with the standard cost–benefit logic of the model. In contrast, women systematically exceed the equilibrium level of information acquisition. This divergence admits several, non-mutually exclusive interpretations: it may reflect a deviation from equilibrium behavior, over-investment relative to the private optimum, or welfare-enhancing effort when individual incentives do not fully internalize group benefits. We find no evidence of gender differences in cognitive ability, and men and women behave similarly conditional on being informed or uninformed, indicating that the primary difference arises at the stage of information acquisition. Evidence from both incentivized decisions and post-experiment survey responses suggests that women place greater weight on the social returns to being informed, linking individual behavior to aggregate performance in a manner not fully captured by the symmetric equilibrium benchmark.

A related question is whether the observed increase in decision accuracy translates into higher aggregate welfare once information acquisition costs are taken into account. In our setting, welfare consequences depend critically on the interaction between *realized* information costs and the likelihood of being pivotal. As information costs decrease, more individuals become informed, which in turn reduces the probability that any single individual is pivotal. At the other extreme, when information costs are prohibitively high, few individuals become informed, and pivotality of informed voting is again low. Consequently, only when information costs are moderate and pivotality is sufficiently high does additional information acquisition increase the probability of correct collective decisions in a way that outweighs the incurred costs, thereby improving both aggregate welfare and individual expected payoffs. Consistent with this tradeoff, the behavioral patterns we document are concentrated in parameter regions where information acquisition is neither trivial nor prohibitively costly (Figure 6). These results suggest that the higher information acquisition observed among women is, in the relevant regions of the

parameter space, aligned with welfare-improving outcomes rather than merely reflecting accuracy-enhancing but inefficient effort.

Implications

An essential contribution of our study is that we relax the assumptions commonly embedded in existing theoretical frameworks—such as women and men *inherently* preferring different policies or possessing distinct substantive expertise. Our study explores and identifies a more general micro-foundation and gender-neutral mechanisms that explain not only *whether* behavioral differences between women and men aggregate into group decisions, but also *how* and *when* it improves the quality of group decision-making. We propose an alternative theory that women's contributions to group decision-making stem not necessarily from specific topic areas, but likely from differences in how men and women approach decision tasks. Thus, our research represents a distinct and powerful contribution that complements existing explanations, offering potential insights into the seemingly contradictory results observed in previous studies.

Specifically, *Informed Representation* emphasizes the significance of an informed majority, aligning with the effects of women's participation and authority discussed by past work. Women contribute to addressing (women's) issues because they devote more attention than men to these issues (Reingold and Swers 2011; Volden, Wiseman and Wittmer 2018), resulting in a higher likelihood of being informed. Women are more likely to participate when they possess the essential knowledge about the question at hand (Payson, Fouirnaies and Hall 2023). According to our theory and experimental results, women use their power with more caution. An interpretation of *Informed Representation* is that in group decision-making, the cost of information acquisition may be correlated with gender composition, and women may be more willing to participate in deliberation when they are more confident of their informedness. If so, an implication of our theory is that women's numbers matter not only because of the social effects of holding the majority, but also because the overall level of informedness is increased and the cost of acquiring

information expertise is lowered within a predominately female group. This suggests a new perspective on why women's numbers matter.

Our theory also suggests that the share of women and men in group does not mechanically contribute to group outcomes, which echoes the findings of Karpowitz et al. (2024) and Mendelberg, Karpowitz and Goedert (2014). We argue that having an informed majority is critical. This implies that gender composition's impact may be nonlinear. Indeed, Mendelberg, Karpowitz and Goedert (2014) find that under majority voting the effect of gender composition is significant when women's number increases from one to three in a five-member group, but not when the number of women increases from three to five. Our theory complements these explanations by highlighting that an informed majority consistently drives desirable outcomes. Thus, organizational designs should promote not only diverse membership but also informed participation across group members.

Informed Authority adds to past work demonstrating that women serving critical roles in governance yield positive outcomes. Instead of solely emphasizing gender-based value differences and/or selection effects, we underscore the importance of information expertise in discussions of leadership and power structures. Our work extends previous work which finds that the unanimity rule helps women's participation and authority when they are in the minority. Relative to previous work, we demonstrate that whether a veto player holds information expertise will decide the quality of group decision-making. *Uninformed veto* may negatively affect the expected positive effect of veto power, suggesting a tradeoff between the unanimity rule and desirable outcomes.

On one hand, the unanimity rule promotes equity since every member is effectively a veto player, which helps women's voice being heard even if they are the minority. On the other hand, extensive studies show that the unanimity rule is less efficient and often generates socially suboptimal outcomes than the simple majority rule (Feddersen and Pesendorfer 1998; Austen-Smith and Feddersen 2006). *All else equal*, the quality of group decision-making decreases as the number of uninformed veto players increases. When women are in the minority, even if the unanimity rule helps them voice, the group

outcome is not necessarily better than under the majority rule. Bouton, Llorente-Saguer and Malherbe (2018) show that the majority-veto voting rule is more efficient than the unanimity rule meanwhile it protects the minority as the unanimity rule. Our study extends previous work by demonstrating that in certain contexts whether men or women serve as the veto player makes a difference, providing a viable topic for future work.

Though conducted in the lab, our study resembled the naturally occurring group decision-making scenarios in important aspects, as these processes essentially consist of information acquisition and voting. The conclusions drawn from our experiments can provide valuable insights into human behavior at different levels of groups when decision makers face the same tradeoff outside the laboratory (Morton and Williams 2010; Druckman and Kam 2011; Falk and Heckman 2009; Snowberg and Yariv 2021). The behavioral differences between women and men identified in our study offer substantive implications that can extend to the understanding of gender effects in groups that require information acquisition (Potters and Van Winden 2000; Fréchette 2015; Kertzer and Renshon 2022).

Our findings also open a novel set of questions for political scientists. Are women who exert significant effort in pre-meeting activities more likely to participate during deliberations? To what extent are informed members affected by "loud" but uninformed members' opinions during group meetings, and are there gender differences in this regard? What is the relative weight of individual information acquisition (before group meetings) compared to collective information acquisition (during group meetings) for women and men, respectively? These remain fascinating open questions and suggest the need for field studies to test our hypotheses in a less controlled setting.

Overall, informed empowerment examines how gender differences in information acquisition influence the functioning of group decision-making, complements existing theories by encompassing the meaningful impact of women's presence beyond gender-specific contexts, and offers insights on when gender differences make a difference to group outcomes. We highlight that information empowers group members such that gendered

differences in information acquisition affect the effects of representation and authority, which introduces a distinctive and potent dimension that stands as a promising avenue for future scholarly exploration.

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Supplementary Information: Informed Empowerment*

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A Models and Theoretical Analysis

We examine the behavior of fully rational group members and investigate the key factors that affect political outcomes through collective decision-making. Voting behavior can then be analyzed through a pivotal voting model. Consider a group in which there are N individuals deciding between two alternatives, $w \in \{\mathbf{1}, \mathbf{2}\}$, who make a collective decision by holding a vote. There are two equally likely states of the world, $\omega \in \{\mathbf{1}, \mathbf{2}\}$, and group members receive a payoff of $\overline{u}$ if alternative $w = \omega$, and 0 otherwise. Thus, the goal of the group is to collectively choose an alternative that matches the state of the world.

First, nature randomly selects $\omega = \mathbf{1}$ or $\omega = \mathbf{2}$ as the state of the world with equal common prior probability for each, $\pi = \pi_1 = \pi_2 = 0.5$. While π is common knowledge, the realization of the state is unknown to the group. Each group member can independently exert costly effort to acquire information about ω. An individual either obtains a private signal or not. If an individual obtains a private signal, it informs the individual about ω with a likelihood of p. As a result, even if ω is the same for every member of the group, the informed group members may still have different understanding about ω because of the moderate uncertainty inherent in the private signal.

Whether a group member i is informed of ω depends on two factors: (1) the effort $e_i \in \mathbb{R}_{\geq 0}$ exerted by the individual during the information acquisition process and (2) the level of difficulty in obtaining information. For simplicity, we introduce an information cost variable c_i, representing the difficulty of acquiring information. This cost c_i is independently and equally likely drawn from a uniform distribution on the interval $[\underline{u}, \overline{u}]$. If $e_i \geq c_i$, then individual i acquires a private signal about ω and incurs a cost of c_i. Conversely, if $e_i < c_i$, the individual remains uninformed about ω and does not incur any cost c_i.

Following the realization of private information, everyone in the group, informed or not, independently and simultaneously votes at no cost for **1** or for **2**. As a benchmark, in *Majority Voting* (hereafter, MV), the alternative with the majority of votes is the group's choice. The timing of the *Majority Voting* game is as follows:

1. The state ω and private information costs are independently drawn.
2. Each group member decide $e_i \in \mathbb{R}_{\geq 0}$.
3. Each group member, whether informed ($e_i \geq c_i$) or uninformed ($e_i < c_i$), decide $w_i \in \{\mathbf{1}, \mathbf{2}\}$.
4. The collective decision w_g is reached and payoffs are received.

We define the *quality of group decision-making* as the likelihood that the group collectively chooses the policy that matches the state of the world

$$Q = \Pr(w_g = 1 | \omega = 1) = \Pr(w_g = 2 | \omega = 2).$$

We begin our analysis by examining how the level of information expertise among group

members affects the voting group's collective choice.

Our analysis focuses on the following equilibrium strategies: Informed group members will follow their private signal to vote; uninformed group members will randomly and equally likely vote for alternative **1** or **2**. There are two reasons for us to perform this analysis. First, voting according to one's signal is a weakly dominant strategy because in pivotal voting the expected benefits of voting following one's signal outweigh the expected benefits of voting against one's signal. Second, because the common prior is $\pi = \pi_1 = \pi_2 = 0.5$, uninformed members do not have incentives to favor either alternative with a likelihood of more than 50%.

When we normalize e_i and c_i, and because c_i is randomly and independently drawn from a uniform distribution, an individual's effort for information maps into a function that decides the likelihood $\rho \in [0, 1]$ that the individual receives the private signal: $\rho(e) = e$. The quality of group decision-making, then, can be written as

$$Q(\rho) = \sum_{\theta=0}^{N} \binom{N}{\theta} \rho^{\theta}(1-\rho)^{N-\theta} r_{\gamma}(\theta) \tag{A.1}$$

, where θ indicates the number of informed members and r_{γ} is likelihood of correct state identification conditional on θ and the voting rule of political processes γ. Specifically,

$$r_{\gamma}(\theta) = \Pr(w = \omega | \theta, \gamma) = \sum_{\alpha=0}^{\theta} p^{\alpha}(1-p)^{\theta-\alpha} \sum_{\beta=0}^{N-\theta} \pi^{\beta}(1-\pi)^{N-\theta-\beta} \tag{A.2}$$

, where α indicates how many informed members voted correctly and β denotes how many uninformed members voted correctly such that $\alpha + \beta \geq \frac{N+1}{2}$. At given parameterizations of θ and γ, the value of $r_{\gamma}(\theta)$ is fixed. Therefore:

Lemma 1 *The quality of group decision-making is strictly increasing in the level of information expertise among group members, determined by the level of effort exerted in the information acquisition process.*

Proof: Notice that $Q(\rho)$ depends on the cumulative distribution function of a binomial random variable with parameter $\rho(e)$. Applying the definition of first-order stochastic dominance to expression (A.1) implies that $Q(\rho)$ is strictly increasing in the effort e that individuals exert to acquire costly information about ω. ■

A.1 Benchmark: Symmetric BNE Predictions

As a benchmark for our analysis, we calculate the point predictions for the equilibrium behavior. We focus on the symmetric strategy in which voters with the same information and tradeoff follow the same equilibrium strategy. Based on a pivotal voting model, each

individual member is motivated by her assessment of the likelihood that she will be pivotal. In general, a member must consider how many other members have acquired information and voted for the alternative that matches the true state of the world. The symmetric best-response for a group member is characterized by the effort for information e^* that solves

$$Piv(\rho, r_\gamma, e^*) = \rho(e^*). \tag{A.3}$$

The left-hand side is the probability an individual group member is pivotal in voting, and the right-hand side is the function of the private cost of information acquisition that decides the likelihood that an individual is informed. Denote a solution to equation (A.3) by $e^*(\rho, r_\gamma)$, which gives the vector of symmetric Bayesian Nash equilibrium information acquisition cut-offs. Let m be the random variable representing an individual group member's conjecture regarding the number of *other* members who acquired information. In pivotal events, if the member does not acquire information, the likelihood that the member will vote for the matching alternative is $\pi = \frac{1}{2}$ such that the group collectively chooses the matching alternative with probability $\pi = \frac{1}{2}$. If the member acquires information, the Bayesian updated likelihood of voting for the matching alternative is

$$B(p|\alpha, m) = \frac{p^\alpha (1-p)^{m+1-\alpha}}{p^\alpha (1-p)^{m+1-\alpha} + p^{m+1-\alpha}(1-p)^\alpha}.$$

Let $\overline{N} = N - 1$. Solving equation (A.3) becomes solving the following equation:

$$\sum_{m=0}^{\overline{N}} \binom{\overline{N}}{m} \rho^m (1-\rho)^{\overline{N}-m} \sum_{\alpha=0}^{m} \binom{m}{\alpha} p^\alpha (1-p)^{m-\alpha} \binom{\overline{N}-m}{\beta} \pi^{\overline{N}-m} \left[B(\cdot) - \frac{1}{2} \right] = \rho(e^*). \tag{A.4}$$

In our experiment $N = 5$, the private signal reveals the true state of the world $p = 0.9$ of the time and wrong state of the world $1 - p = 0.1$ of the time. We solve equation (A.4) with our parameterizations and get $e^* = 23.53$ and $\rho(e^*) = 0.24$.

A.2 Informed Representation

Drawing from Lemma 1 as our foundational premise, we deduce that for group decision-making to consistently yield favorable outcomes, a pivotal prerequisite is that $\theta > \frac{N}{2}$, signifying that the majority of the group should comprise informed members. This serves as the theoretical underpinning of *Informed Representation*, as elaborated upon in the main text. *All else equal*, it can be anticipated that in a group where informed individuals constitute the majority, there will be a heightened average level of informational expertise compared to one where informed members are the minority.

Proposition 1 (Informed Representation) *Under Majority Voting, the group in which the number of informed individuals θ_1 satisfies $\theta_1 > \frac{N}{2}$ consistently identify ω with a higher likelihood than the group in which the number of informed individuals θ_2 satisfies $\theta_2 < \frac{N}{2}$.*

Proof: When $\theta_1 > \frac{N}{2}, \theta_1 > N - \theta_1$, the likelihood of correct state identification is

$$r_{MV}(\theta_1) = \Pr(w = \omega|\theta_1) = \sum_{\alpha=0}^{\theta_1} p^{\alpha}(1-p)^{\theta_1-\alpha} \sum_{\beta=0}^{N-\theta_1} \pi^{\beta}(1-\pi)^{N-\theta_1-\beta}.$$

When $\theta_2 < \frac{N}{2}, \theta_2 < N - \theta_2$, the likelihood of correct state identification is

$$r_{MV}(\theta_2) = \Pr(w = \omega|\theta_2) = \sum_{\alpha=0}^{\theta_2} p^{\alpha}(1-p)^{\theta_2-\alpha} \sum_{\beta=0}^{N-\theta_2} \pi^{\beta}(1-\pi)^{N-\theta_2-\beta}.$$

Since $\theta_1 > \theta_2$, $p > 0.5$, and $\pi = 0.5$, $r_{MV}(\theta_1) > r_{MV}(\theta_2)$. ■

Based on Lemma 1 and Proposition 1, we introduce gender differences in the willingness to exert effort for information expertise and simulate the relationship between the quality of group decision-making, effort for information expertise, and the level of informedness. We assume that women will exert more effort than men, $e^{women} = \delta e^{men}$, where $\delta > 1$, such that women are more likely to be informed than men. We consider **4M1W** (4 men 1 woman) and **2M3W** (2 men and 3 women), representing situations where women constitute the minimum minority and the majority, respectively. The simulated results are presented in Figure A1. As anticipated, a group in which women are the majority is more likely to collectively and correctly identify ω than a group in which women are the minority.

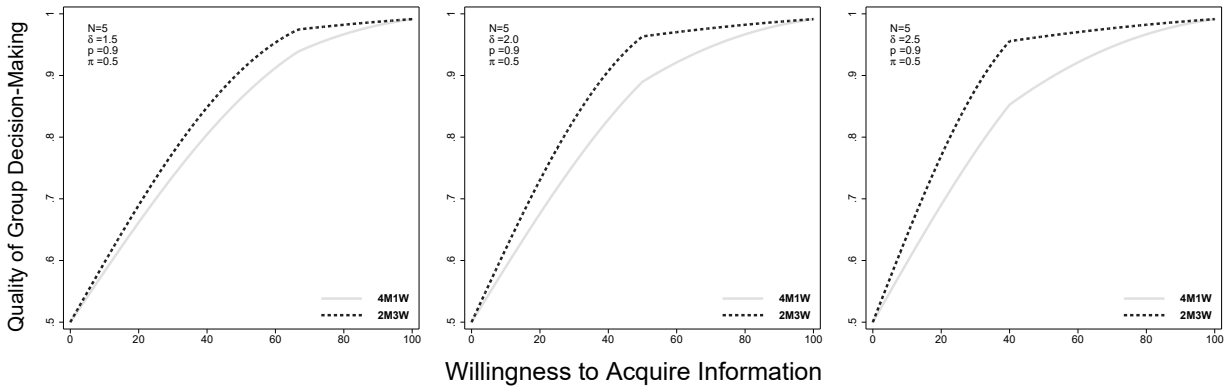


Figure A1: Effects of Informed Representation

A.3 Informed Authority

We now consider *Majority Voting with Veto* (hereafter, MVV) in which someone from the group has veto power (i.e., the veto player) and the group's choice is determined by the majority's choices and whether veto is used. The timing of the *Majority Voting with Veto* game is as follows:

1. The state ω and private information costs are independently drawn.
2. Each group member, including the veto player, must decide $e_i \in \mathbb{R}_{\geq 0}$.
3. Each group member, whether informed ($e_i \geq c_i$) or uninformed ($e_i < c_i$), decide $w_i \in \{\mathbf{1}, \mathbf{2}\}$.
4. The veto player is told the w_g and decides whether to veto.
5. The collective decision is reached and payoffs are received. Veto changes the group's decision only when the majority's votes are for alternative **1**.

Let $r_{MVV}(v = 1)$ denote the probability of correct state identification under *Majority Voting with Veto* when the veto player is informed, and $r_{MVV}(v = 0)$ denote the probability when the veto player is uninformed, where $v \in \{0, 1\}$ represents whether the veto player is informed or not.

Proposition 2 (Informed Authority) $r_{MVV}(v = 1) > r_{MVV}(v = 0)$.

Proof: Case 1: If $v = 1$, the veto player possesses information about ω. This means they can make an informed decision about whether to use the veto based on their private information. If the majority's votes are correct, the veto player does not need to do anything, ensuring that the correct decision is made.
Case 2: If $v = 1$ and the majority's votes are incorrect, the veto player can veto, preventing an incorrect decision from being made.
Case 3: If $v = 0$, the veto player lacks information about ω. In this case, they do not have the ability to make an informed decision about whether to use the veto, and they should not use veto but count on the majority's choices, because uninformed veto is (weakly) dominated. Taken together, having an uninformed veto player provides no advantage in terms of making the correct decision. This concludes the proof. ■

Based on Proposition 2, we introduce gender differences in the veto players' willingness to exert effort for information expertise and simulate the relationship between the quality of group decision-making and the level of informedness. We assume that women veto players will exert more effort than regular men group members, $e_{v=1}^{women} = \delta e_{v=0}^{men}$, where $\delta > 1$, such that women veto players are more likely to be informed than regular men group members. We also assume that men veto players will exert more effort than regular men group members, $e_{v=1}^{men} = \zeta e_{v=0}^{men}$, where $\zeta > 1$, such that men veto players are more likely to be informed than regular men group members. We consider **MVeto-4M1W** (4 men 1 woman, the veto player is a man) and **WVeto-4M1W** (4 men and 1 women, the veto player is a woman). The simulated results are presented in Figure A2. As anticipated, *all else equal* and when there are four men and one woman in the group, a group in which a woman serves as the veto player is more likely to identify ω than a group in which a man serves this role.

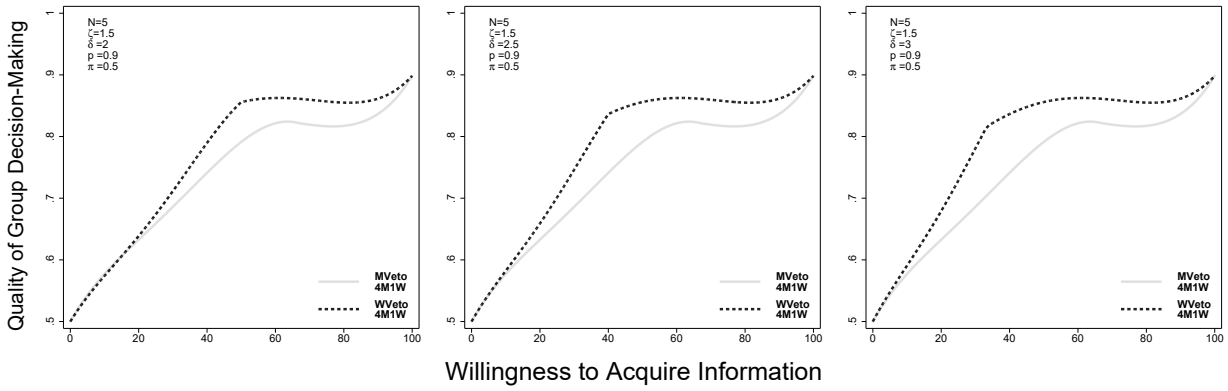


Figure A2: Effects of the Critical Role Mechanism

B Experimental Results

B.1 Non-parametric Analysis

We report the parametric and regression-based analysis in the main text. We also conducted arguably more conservative non-parametric tests to examine the robustness of these findings. Fréchette (2012) discusses the advantages and disadvantages of both approaches. Reporting both shows that our main results are robust to the statistical approach.

In the regressions, the data analysis is always clustered at the group level. The clustering is used to account for arbitrary correlation between observations within a given group. In the non-parametric analysis, to approach the clustering analysis used in the regressions, following Fréchette (2012), we use group averages as the unit of analysis.

For the comparisons between experimental conditions, we use the non-parametric Fisher-Pitman permutation tests, which rely on fewer and weaker assumptions and have the highest power (100%) compared to related tests (Siegel 1957). Using a Monte-Carlo study, Moir (1998) shows the Fisher-Pitman permutation tests have statistically reliable power for as few as eight observations per treatment category. For the comparisons of matched data, we use the non-parametric signed-rank tests. We have 16 groups in Experiment I (eight groups for each experimental condition) and 16 groups in Experiment II (eight groups for each experimental condition).

Table A1 replicates the key findings from the paper except using non-parametric tests on group averages. This leads to the same main conclusions as those drawn from its counterpart in the paper.

Table A1: The Quality of Group Decision-Making – Non-Parametric Tests

	Comparisons	Effect	$p-$value
Exp. I	**2M3W – 4M1W**	↗	0.044
Exp. II	**WVeto-4M1W – MVeto-4M1W**	↗	0.002
	WVeto-4M1W† **– MVeto-4M1W**†	n.s.	0.162†
	WVeto-4M1W – WVeto-4M1W†	↗	0.025
	MVeto-4M1W – MVeto-4M1W†	n.s.	0.778
Exp. II vs Exp. I	**WVeto-4M1W – 4M1W**	↗	0.003
	MVeto-4M1W – 4M1W	n.s.	0.768

Notes: The p-values are the results of two-tailed tests.
† If we estimate group outcomes based on individual members' vote before veto is used, then with the same set of participants, the quality of group decision-making is about 0.650 in MVeto-4M1W and 0.738 in WVeto-4M1W, which is not significantly different.

B.2 Comparisons of Behavior Across Experiments

In Table A2, we report information acquisition behavior by gender and group role.

Table A2: Group Members' Effort by Experiment

	Dependent Variable: Effort for Information			
	Regular Members		Veto Players	
	Men	Women	Men	Women
Variables	(1)	(2)	(3)	(4)
Experiment II	-0.654	-12.120		
	(6.038)	(11.456)		
Experiment III		4.180	-14.833	-14.733
		(9.537)	(9.320)	(10.454)
Constant	20.614***	50.183***	45.294***	85.144***
	(3.880)	(5.789)	(7.806)	(9.345)
Exp. II vs *Exp. III*		-16.300		
		(12.456)		
Number of participants	104	52	32	32
Clustered S.E.	Yes	Yes	Yes	Yes

Notes: OLS specification. *Experiment II* is a binary indicator that represents whether subjects participated in Experiment II. *Experiment III* is a binary indicator that represents whether subjects participated in Experiment III. Columns 1 and 2 include observations of regular group members' choices. Columns 3 and 4 report only the results of veto players who participated in Experiments II and III. Robust standard errors are clustered at the group level and reported in parentheses.
***, **, and * denote significance at the 1%, 5%, and 10% levels. *Takeaway*: The information acquisition behavior of regular group members and veto players is statistically similar across experiments.

B.3 Post-Experiment Survey

Before revealing participants' earnings or the results from the three parts of the experiment, participants were asked to answer a short post-experiment survey. Based on the results reported in Table A3, we find that information acquisition was significantly more important to women than men. Veto players express that information acquisition is significantly more important to them than regular group members. These survey results are consistent with the main findings that both for men and women, veto players exert significantly more effort to acquire information than regular group members. We also find that in the process of making information acquisition and voting decisions, women care more about the group's total benefits than men. Moreover, veto players express that they feel their vote is pivotal to the group outcome at a significantly higher level than regular group members. These survey results are consistent with the main findings that both for men and women, veto players exert significantly more effort to acquire information than regular group members.

Table A3: Summary Statistics of Post-Experiment Survey

	Pooled			Women			Men		
	Men	Women	$p-$value	Regular Member	Veto Player	$p-$value	Regular Member	Veto Player	$p-$value
Information	4.257	5.964	$p_t < 0.001$ $p_{FP} < 0.001$ $p_{KS} < 0.001$	5.519	6.688	$p_t = 0.015$ $p_{FP} = 0.029$ $p_{KS} = 0.087$	3.904	5.406	$p_t < 0.001$ $p_{FP} = 0.002$ $p_{KS} < 0.001$
Welfare	4.522	6.524	$p_t < 0.001$ $p_{FP} < 0.001$ $p_{KS} < 0.001$	6.481	6.594	$p_t = 0.804$ $p_{FP} = 0.830$ $p_{KS} = 0.984$	4.413	4.875	$p_t = 0.332$ $p_{FP} = 0.350$ $p_{KS} = 0.511$
Pivotality	4.868	6.143	$p_t < 0.001$ $p_{FP} < 0.001$ $p_{KS} < 0.001$	5.635	6.969	$p_t = 0.002$ $p_{FP} = 0.002$ $p_{KS} = 0.024$	4.548	5.906	$p_t = 0.002$ $p_{FP} = 0.001$ $p_{KS} = 0.022$

Notes: We report the mean value of individuals' responses by gender and role in the group.
Information: "How important was it to you to acquire information?" This question was measured based on a scale of 1-9, 1 being not important at all, 5 being neutral, and 9 being extremely important.
Welfare: "When you make information acquisition and voting decisions, to what extent you think about your own benefits and to what extent you think about the group's total benefits?" This question was also measured based on a scale of 1-9, 1 being only thinking about own benefits and not thinking about group's total benefits at all, 5 being thinking about own benefits and group's total benefits equally, and 9 being only thinking about group's total benefits and not thinking about own benefits at all.
Pivotality: "To what extent do you think your vote affects the voting outcomes?" This question was again measured based on a scale of 1-9, 1 being thinking the vote does not affect the outcomes at all, 5 being neutral, and 9 being thinking the vote is decisive and pivotal.
We perform a regression to investigate the difference of means. Individual-level observations are nested within groups, so we cluster robust standard errors by group, and the corresponding p-value is denoted by p_t. We also use a Fisher-Pitman permutation test, an analogous non-parametric test, to examine the difference of means, and the corresponding p-value is denoted by p_{FP}. We conduct a Kolmogorov-Smirnov test, an non-parametric test, to check the distribution of individuals' responses, and the corresponding p-value is denoted by p_{KS}.
Takeaway: Information acquisition was significantly more important to women than men. Veto players express that information acquisition is significantly more important to them than regular group members. In the process of making information acquisition and voting decisions, women care more about the group's total benefits than men. Moreover, veto players express that they feel their vote is pivotal to the group outcome at a significantly higher level than regular group members. Our results are robust to different statistical methods.

B.4 Gender Differences in Effort for Information

Individual-level observations are nested within groups, so we report cluster robust standard errors. Cell entries are ordinary least squares regression coefficients; cluster-robust standard errors in parentheses (clustered by group). Even after we have controlled for the influence caused by demographic factors, as reported in Tables A4, our findings are consistent and qualitatively identical.

B.5 Distribution of Effort and Preferences

The gender differences observed in our experiment are systematic and consistent. We find no evidence that these average differences, or their distributions (see Figure A3), are significantly influenced by specific outliers, broadly defined. Whether using non-parametric permutation tests (via random shuffling) or parametric tests, our main findings remain qualitatively unchanged.

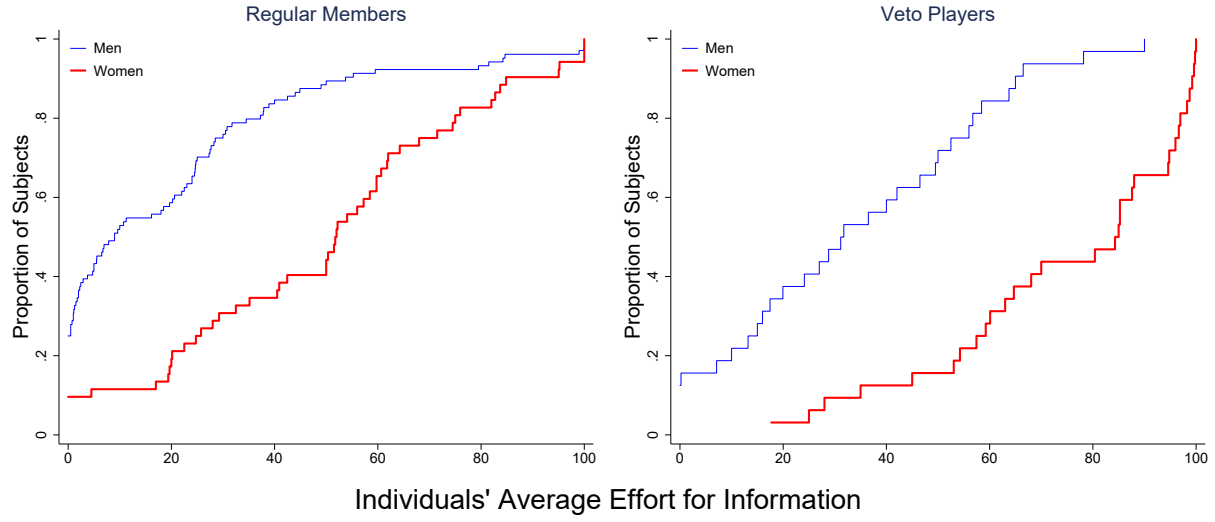


Figure A3: Cumulative Distribution of Effort by Voter Type

B.6 Determinants of Individuals' Effort for Information

In Table A5, we report regression results of factors that affect individuals' effort decisions.

Table A4: Gender Differences in Effort for Information Expertise

	Dependent Variable: Effort for Expertise								
	Experiment I		Experiment II			Experiment III		Pooled	
Variables	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Women	29.569***	31.689***	38.477***	18.103	22.810*			29.021***	29.576***
	(6.255)	(6.709)	(9.750)	(10.946)	(11.898)			(4.906)	(4.857)
VetoPlayer				25.334**	25.764**			13.907**	14.285***
				(9.409)	(9.992)			(5.414)	(5.381)
Women×*VetoPlayer*				21.747	25.266			10.904	12.958
				(18.115)	(18.045)			(8.105)	(8.267)
Constant	20.614***	5.037	23.127***	19.960***	-14.414			20.262***	-4.214
	(3.945)	(22.631)	(4.164)	(4.706)	(28.417)			(2.034)	(17.401)
Women veto players versus Men veto players				39.850***	48.076***	39.950***	39.892***	39.925***	42.534***
				(12.380)	(11.347)	(6.868)	(6.868)	(6.165)	(6.218)
Women veto players versus Women regular members				47.081***	51.031***			24.811***	27.243***
				(13.842)	(15.146)			(6.094)	(6.141)
Men veto players versus Men regular members				25.334**	25.764**			13.907**	14.285***
				(9.409)	(9.992)			(5.414)	(5.381)
Number of participants	80	80	80	80	80	60	60	220	220
R-squared	0.198	0.303	0.176	0.300	0.404	0.228	0.328	0.277	0.330
w/ Control Variables	No	Yes	No	No	Yes	No	Yes	No	Yes
Clustered S.E.	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: OLS specification. Robust standard errors are clustered at the group level and reported in parentheses.
***, **, and * denote significance at the 1%, 5%, and 10% levels.
Control variables include *Age*, *Academic Major*, *Ethnicity*, *GPA*, and *Cognitive Ability* (measured by the Cognitive Reflection Task, Frederick 2005. Participants were asked to respond to the following three non-incentivized questions with no time limit in the post-experiment survey: (1) A bat and a ball cost $1.10 in total. The bat costs a dollar more than the ball. How much does the ball cost? [Answer: 5 cents] (2) If it takes 5 machines 5 minutes to make 5 widgets, how long would it take 100 machines to make 100 widgets? [Answer: 5 minutes] (3) In a lake, there is a patch of lily pads. Every day, the patch doubles in size. If it takes 48 days for the patch to cover the entire lake, how long would it take for the patch to cover half of the lake? [Answer: 47 days]). The default major is *Natural Science*. The default race is *White*. None of our participants reported her/him/them as *Native American* or *American Indian*.

Table A5: Determinants of Individuals' Effort for Information

	Dependent Variable: Effort for Information													
	Regular Group Members							Veto Players						
Variables	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)
Collaboration	0.657***					0.495***	0.408***	0.806***					0.564***	0.387***
	(0.130)					(0.127)	(0.115)	(0.142)					(0.139)	(0.112)
Risk		-0.318***				-0.213***	-0.181***		-0.259**				0.085	0.035
		(0.063)				(0.062)	(0.057)		(0.109)				(0.096)	(0.117)
Information			5.122***			3.491***	3.449***			9.904***			5.116**	5.635***
			(0.822)			(0.742)	(0.741)			(1.657)			(2.375)	(1.642)
Welfare				3.142***		-0.511	-1.179				7.445***		1.912	1.250
				(1.150)		(1.053)	(1.084)				(1.518)		(1.650)	(1.636)
Pivotality					4.557***	0.168	0.214					6.064***	0.456	-0.051
					(1.002)	(1.067)	(0.999)					(1.683)	(1.745)	(1.578)
# participants	156	156	156	156	156	156	156	64	64	64	64	64	64	64
R-squared	0.168	0.094	0.160	0.051	0.065	0.292	0.403	0.268	0.046	0.226	0.175	0.097	0.372	0.471
Controls	No	No	No	No	No	No	Yes	No	No	No	No	No	No	Yes
Clustered S.E.	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: OLS specification. Robust standard errors are clustered at the group level and reported in parentheses.
Collaboration preference is measured by individuals' contribution to an incentivized threshold public good. *Risk* preference is measured by individuals' investment to an incentivized risky option. *Information* Importance is measured by individuals' response (on a scale of 1-9) to the survey question "How important was it to you to acquire information?" *Welfare* consideration is measured by individuals' response (on a scale of 1-9) to the survey question "When you make information acquisition and voting decisions, to what extent you think about your own benefits and to what extent you think about the group's total benefits?" *Pivotality* is measured by individuals' response (on a scale of 1-9) to the survey question "To what extent do you think your vote affects the voting outcomes?"
Control variables include *Gender*, *Age*, *Academic Major*, *Ethnicity*, *GPA*, and *Cognitive Ability* (measured by the Cognitive Reflection Task, Frederick 2005. Participants were asked to respond to the following three non-incentivized questions with no time limit in the post-experiment survey: (1) A bat and a ball cost $1.10 in total. The bat costs a dollar more than the ball. How much does the ball cost? [Answer: 5 cents] (2) If it takes 5 machines 5 minutes to make 5 widgets, how long would it take 100 machines to make 100 widgets? [Answer: 5 minutes] (3) In a lake, there is a patch of lily pads. Every day, the patch doubles in size. If it takes 48 days for the patch to cover the entire lake, how long would it take for the patch to cover half of the lake? [Answer: 47 days]). The modal major is *Natural Science*. The modal race is *White*. None of our participants reported her/him/themselves as *Native American* or *American Indian*.
***, **, and * denote significance at the 1%, 5%, and 10% levels.

B.7 Individual Behavior

In Table A6, we focus on the behavioral analysis of how individuals make voting decisions conditional on whether they are informed, whether they are regular group members or veto players, and whether women or men hold the veto power. In Table A7, we study aggregate gender differences in using vetoes. In Table A8, we further break down the analysis of veto players' use of veto by their own choice, the majority's choice, and whether they are informed. In Table A9, we explore the determinants of veto players' uninformed veto.

Table A6: The Likelihood of Voting for Jar 1 by Informedness, Role, and Gender

	Dependent Variable: Pr(Voting Jar 1)					
	Regular Member	Veto Player	p-value	Men	Women	p-value
Informed $\omega = 1$	0.906	0.906	n.s.	0.931	0.890	n.s.
Informed $\omega = 2$	0.075	0.081	n.s.	0.107	0.062	n.s.
Uninformed	0.565	0.593	n.s.	0.581	0.541	n.s.

Takeaway: Individuals in general follow their acquired information to vote when informed. When they are uninformed, they vote for Jar 1 with a likelihood that is statistically identical to 50%. There is no significant difference in this behavioral pattern between regular group members and veto players, or between men and women.

Table A7: Aggregate Level Gender Differences in Using Veto Power

	Dependent Variable: Veto = 1		
Variables	(1)	(2)	(3)
Women	-0.089**	-0.101*	
	(0.042)	(0.057)	
Uninformed		-0.097**	
		(0.047)	
Women$\times$*Uninformed*		-0.112*	
		(0.058)	
Constant	0.255***	0.322***	0.286***
	(0.032)	(0.045)	(0.087)
Pr(Veto|Uninformed), women relative to men		-0.213***	
		(0.034)	
Pr(Veto|Informed $\omega = 1$), women relative to men			-0.197**
			(0.093)
Number of participants	64	64	64
Clustered S.E.	Yes	Yes	Yes

Notes: LPM specification. We use linear probability models to report the results of regression based analysis because it facilitates the presentation of linear combined effects as shown in this table. In Column 3, we narrow down our investigation into the cases where they are informed that the state of the world is Jar 1 so that their veto can not change the majority's vote. Robust standard errors are clustered at the voting group level and reported in parentheses. ***, **, and * denote significance at the 1%, 5%, and 10% levels. *Takeaway*: Women use veto power almost only when they are informed. Men use veto power even if they are uninformed. Even if the use of veto is not consequential, men are significantly more likely to use the power anyway.

Table A8: The Likelihood of Using Veto by Own Choice and Majority's Choice

	Majority Choice $w_g = 1$		Majority Choice $w_g = 2$	
	Men	Women	Men	Women
Informed $\omega = 1$, own choice $w_i = 1$	0.04 (54)	0.08 (135)	0.75 (28)	0.00 (44)
Informed $\omega = 1$, own choice $w_i = 2$	1.00 (1)	0.54 (13)	0.00 (1)	0.00 (12)
Informed $\omega = 2$, own choice $w_i = 1$	0.86 (7)	0.56 (16)	0.00 (5)	0.00 (3)
Informed $\omega = 2$, own choice $w_i = 2$	0.79 (29)	0.90 (72)	0.15 (74)	0.07 (176)
Uninformed, own choice $w_i = 1$	0.18 (204)	0.03 (59)	0.30 (64)	0.00 (35)
Uninformed, own choice $w_i = 2$	0.47 (55)	0.00 (23)	0.15 (118)	0.00 (52)

Note: Number of observations are given in parentheses. *Takeaway*: All else equal, men in general are more likely to use veto regardless of whether the veto decision is constructive.

Table A9: Determinants of Individuals' Uninformed Veto

	Dependent Variable: Uninformed Veto = 1							
Variables	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Collaboration	-0.002***						-0.002***	-0.001*
	(0.000)						(0.000)	(0.000)
Risk		0.001***					0.001***	0.000
		(0.000)					(0.000)	(0.000)
Information			-0.014				-0.003	-0.005
			(0.009)				(0.008)	(0.004)
Welfare				-0.011			0.000	0.005
				(0.007)			(0.006)	(0.003)
Pivotality					-0.007		0.004	0.004
					(0.007)		(0.005)	(0.003)
$w_i \neq w_g$						0.053**	0.043**	0.030***
						(0.024)	(0.018)	(0.011)
# participants	64	64	64	64	64	64	64	64
log-likelihood	-324.9	-333.9	-347.2	-348.2	-351.5	-347.5	-307.1	-267.6
Controls	No	No	No	No	No	No	No	Yes
Clustered S.E.	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Probit specification. Each column reports average marginal effects from Probit regressions.
Collaboration preference is measured by individuals' contribution to an incentivized threshold public good. *Risk* preference is measured by individuals' investment to an incentivized risky option. *Information* Importance is measured by individuals' response (on a scale of 1-9) to the survey question "How important was it to you to acquire information?" *Welfare* consideration is measured by individuals' response (on a scale of 1-9) to the survey question "When you make information acquisition and voting decisions, to what extent you think about your own benefits and to what extent you think about the group's total benefits?" *Pivotality* is measured by individuals' response (on a scale of 1-9) to the survey question "To what extent do you think your vote affects the voting outcomes?" $w_i \neq w_g = 1$ is a binary indicator that represents whether a veto player's own choice differs from the majority's choice.
Control variables include *Gender*, *Age*, *Academic Major*, *Ethnicity*, *GPA*, and *Cognitive Ability* (measured by the Cognitive Reflection Task, Frederick 2005. Participants were asked to respond to the following three non-incentivized questions with no time limit in the post-experiment survey: (1) A bat and a ball cost $1.10 in total. The bat costs a dollar more than the ball. How much does the ball cost? [Answer: 5 cents] (2) If it takes 5 machines 5 minutes to make 5 widgets, how long would it take 100 machines to make 100 widgets? [Answer: 5 minutes] (3) In a lake, there is a patch of lily pads. Every day, the patch doubles in size. If it takes 48 days for the patch to cover the entire lake, how long would it take for the patch to cover half of the lake? [Answer: 47 days]). The default major is *Natural Science*. The default race is *White*. None of our participants reported her/him/them as *Native American* or *American Indian*.
Robust standard errors are clustered at the group level and reported in parentheses.
***, **, and * denote significance at the 1%, 5%, and 10% levels.

C Replications and Robustness Check

A critical feature of our experimental design is that we elicit how much effort a decision maker would like to exert for expertise. After a participant reports the willingness to pay for information, the computer randomly generates a private cost of information. If this cost falls below the participant's effort, the participant is not informed of the state of the world. Since whether a participant is informed affects the voting choice, our experimental design implies that the outcome of our experiment, both for men and women, depends on the actual realization of the information cost. A concern might be whether the information costs drawn at random by the computer produced our treatment effects, and thus, our results are due to chance rather than a real effect. Importantly, this is the case in any empirical study.

In this section, we conduct a robustness check and examine to what extent we can replicate the main results with simulations. Specifically, we can simulate a counterfactual experiment where the effort for expertise reported by each experimental subject remains the same, but where the randomly generated information costs are different. To achieve this, we assume that participants will vote for Jar **1** (Jar **2**) when they are informed that the state of the world is Jar **1** (Jar **2**), and equally likely and randomly vote for Jar **1** or Jar **2** when they fail to acquire expertise. This assumption is not arbitrary, but highly consistent with our experimental observations. Based on this decision rule, we conduct a Monte Carlo simulation by redrawing the information cost for each round following the same algorithm used to produce the actual information costs in the experiment.

We use participants' decisions on how much effort they would like to exert for expertise that we observed in the experiments, and this information directly decides the likelihood that they acquired expertise, and therefore, the likelihood of the group collectively chooses the matching policy. Notice that participants did not receive any feedback regarding the randomly generated information costs in the experiment, and thus, the information costs, whether they are generated in the experiment or after the experiment, do not affect participants' decisions to acquire expertise or what to vote for if they are (un)informed. In other words, our experimental design allows us to conduct this replication and robustness check while keeping the conclusions as valid as the conclusions derived from the information costs generated in the experiment. We then compare our estimates of the treatment groups, for the likelihood that the matching policy is collectively chosen by the group. We run this simulation 1000 times and average over these 1000 simulations.

C.1 Informed Representation

The comparisons of the quality of group decision-making between **4M1W** and **2M3W** are illustrated in Figure A4. The estimate is 0.648 in **4M1W** and within the 95% confidence interval of the experimental observation at 0.613. The estimate is 0.7995 in **2M3W** and almost identical to the experimental observation at 0.800. The difference between the simulated results and the experimental observations is about 0.035 in **4M1W** and less than 0.001

in **2M3W,** respectively. None of these differences are significantly different from zero.

Importantly, the effects of *Informed Representation* are robust. Comparing the simulated means, we find that the quality of group decision-making is significantly higher in **2M3W** than in **4M1W** at the 0.001 level, which is consistent with the results reported in the main text. These results suggest that the identified effects of *Informed Representation* are not likely to be driven by stochastic factors.

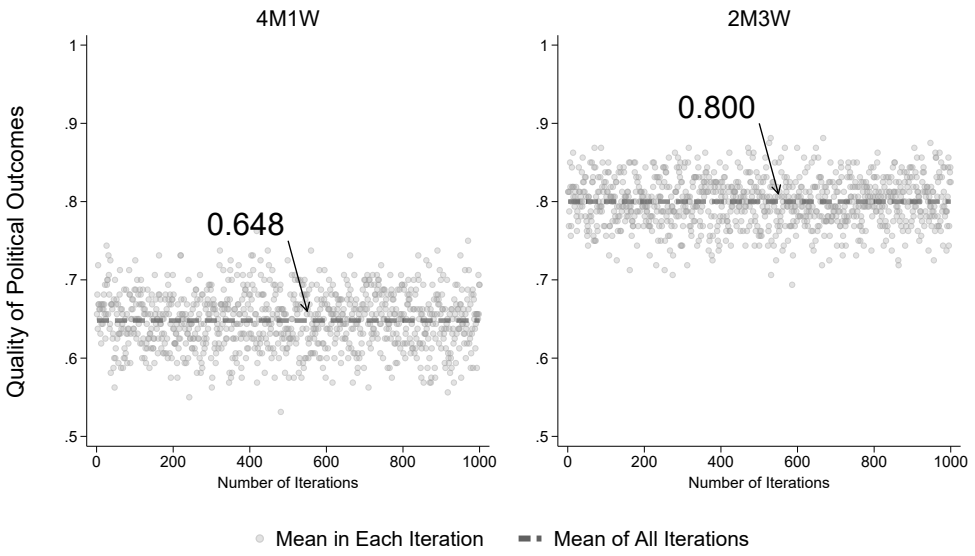


Figure A4: Replications of the Effects of Informed Representation

C.2 Informed Authority

We now turn to replicate *Informed Authority*. Regular group members received no feedback regarding veto players' decisions (i.e., what veto players vote for and whether they have used their veto), but veto players were informed of how many votes there are for Jar 1 and Jar 2 before making a veto decision. Importantly, our design implies that regular group members' behavior is not influenced by veto players' behavior, but veto players' decisions may be affected by the choices of the regular group members. Matching regular group members with different veto players could therefore lead to different outcomes. We have examined and identified that regular group members' behavior is identical between Experiment I and Experiment II. As a result, in Experiment II the focus of our analysis is on veto players' choices.

We keep the decision rule of information acquisition and voting the same as above so that the results of the experimental conditions with veto can be comparable to the experimental conditions without veto. However, because of gender differences in information acquisition and political power usage, we expect in theory and observe in experiments that whether

women or men have veto power affects the political outcomes through collective decision-making. Importantly, women veto almost only when they are informed that the state of the world is Jar **2** while the majority's vote is Jar **1**, so their veto is consequential and highly likely to switch the outcome of the group from low payoffs to high payoffs. But men veto even if they are uninformed or the veto is not consequential (i.e. the state of the world is Jar **1** but the majority's vote is Jar **2** or the majority has voted for Jar **2** regardless of the state of the world).

We incorporate the gender differences in using veto power into the simulation, run this simulation 1000 times, and average over these 1000 simulations. The comparisons of the quality of group decision-making between **WVeto-4M1W** and **MVeto-4M1W** are illustrated in Figure A5. The treatment effect of women having veto power on the quality of group decision-making continues to hold.

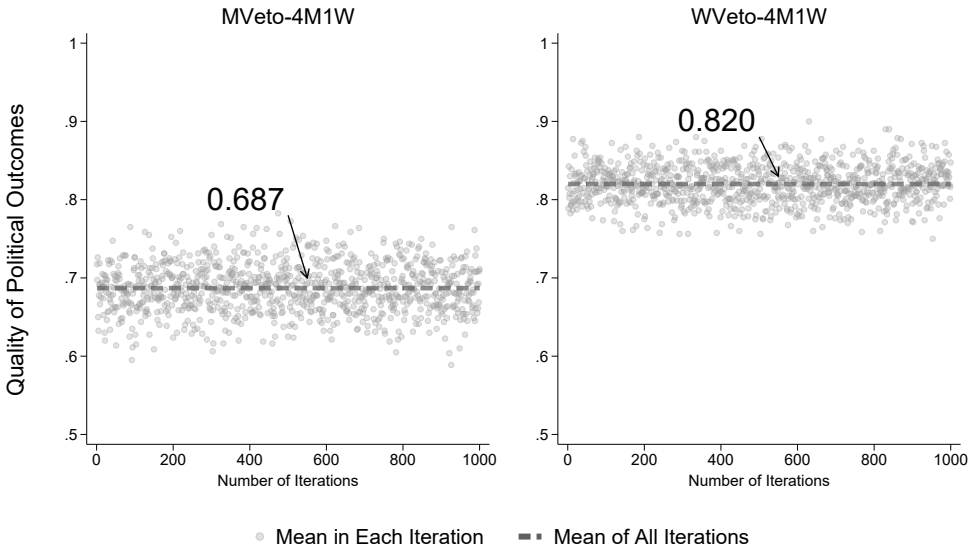


Figure A5: Replications of the Effects of Informed Authority

The estimate is 0.687 in **MVeto-4M1W** and within the 95% confidence interval of the experimental observation at 0.631. The estimate is 0.820 in **WVeto-4M1W** and within the 95% confidence interval of the experimental observations at 0.869. The difference between the simulated results and the experimental observations is about 0.056 in **MVeto-4M1W** and 0.049 in **WVeto-4M1W**, respectively. None of these differences are significantly different from zero.

Comparing the simulated means, we find that the quality of group decision-making is significantly higher in **WVeto-4M1W** than in **MVeto-4M1W** at the 0.01 level, which is consistent with the results reported in the main text. These results suggest that the identified effects of how women improve political outcomes when they play a more critical role in the

political process, such as having veto power, are not likely to be driven by stochastic factors but that they are more likely to make informed voting and veto the majority's uninformed or biased vote.

Finally, we perform the last robustness checks by randomly matching regular group members with different sets of veto players ($N = 64$) and estimating the average quality of group decision-making for **WVeto-4M1W** and **MVeto-4M1W**, respectively. We are able to conduct these robustness checks because veto players and regular group members made decisions under the same set of information and parameterizations in Experiment III as in Experiment II. Regular group members receive no feedback regarding whether the others are informed or how the others voted.

We retrieve the vote choices of regular group members participated in the previous 16 sessions (i.e., Experiment II) and use the same information as feedback for veto players in Experiment III. Thus, matching regular group members with different veto players will not change subjects' choices but different veto players' behavior may affect the group decision and therefore the quality of group decision-making.

To perform this analysis, we conduct a Monte Carlo simulation by redrawing the set of veto players and their corresponding decisions 1000 times. These results are reported in Figure A6. We find that on average the quality of group decision-making is 0.600 in **MVeto-4M1W** but 0.819 in **WVeto-4M1W**, which is qualitatively identical to our main findings.

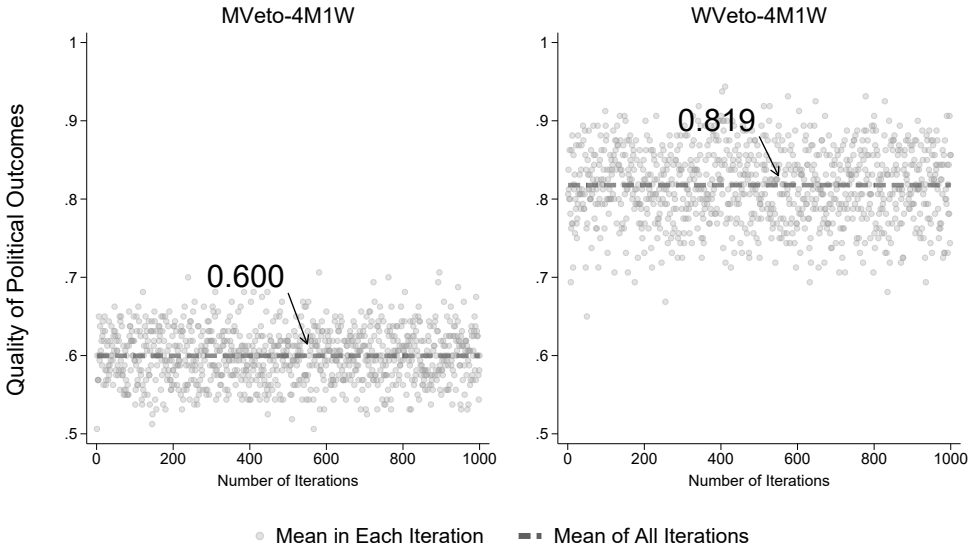


Figure A6: Robustness of Informed Authority's Effects with Rematched Veto Players

D Distribution of Treatment Effects

D.1 Estimation Strategy

Since we elicit individuals' willingness to pay for information when subjects do not yet know their information cost, we can generate and assign specific information costs (from the lowest to the highest) to decision-makers post-experiment using simulations. This enables us to estimate the distribution of treatment effects in a simple and straightforward fashion. Meanwhile, the distribution of treatment effects has the same claim to validity as the average effects reported in the main text, because the only difference is the realization of information cost, which was unobserved by subjects. In other words, the unobserved information cost will not change decision-makers decision rules. In estimating the distribution of effects by varying the information cost, we use a decision rule system that replicates the decision rules used by participants and follows the individual behavioral patterns observed in our experiments (as reported in Appendix B.7).

D.2 Informed Representation

In estimating the distribution of *Informed Representation*'s effects, we use the following decision rules.

Rule 1: When a subject was informed in a decision period and voted for a particular jar in the experiments, then when the subject is informed in the simulations, the subject will vote for that particular jar.

Rule 2: When a subject was informed in the experiments but uninformed in the simulations, the subject will vote for Jar 1 and Jar 2 with the same probability we observed individuals vote for each jar in the experiments.

Rule 3: When a subject was uninformed in the experiments and voted for a particular jar in experiments, then when the subject is uninformed in the simulations, the subject will vote for that particular jar.

Rule 4: When a subject was uninformed in the experiments but informed in the simulations, the subject will follow the signal and vote for the jar shown to them with the likelihood observed in the experiments for men and women.

D.3 Informed Authority

In estimating the distribution of *Informed Authority*'s effects, we use the decision rules for *Informed Representation* but with the following additional decision rules.

Rule 5a: When a veto player was informed in the experiments, the majority's collective choice

was identical to the veto player's choice, and the veto player did use veto, then when the veto player is informed in the simulations, the veto player will veto the majority's collective choice when it is identical to the veto player's vote.

Rule 5b: When a veto player was informed in the experiments, the majority's collective choice was different from the veto player's choice, and the veto player did not use veto, then when the veto player is informed in the simulations, the veto player will not veto the majority's collective choice when it is different from the veto player's vote.

Rule 6a: When a veto player was uninformed in the experiments, the majority's collective choice was identical to the veto player's choice, and the veto player did use veto, then when the veto player is uninformed in the simulations, the veto player will veto the majority's collective choice when it is identical to the veto player's vote.

Rule 6b: When a veto player was uninformed in the experiments, the majority's collective choice was different from their choice, and the veto player did not use veto, then when the veto player is uninformed in the simulations, the veto player will not veto the majority's collective choice when it is different from the veto player's vote.

Rule 7: When a veto player was informed in the experiments but uninformed in the simulations, then in the simulations men veto players will veto the majority's choice with the likelihood observed in the experiments for men and women respectively.

Rule 8: When a veto player was uninformed in the experiments but informed in the simulations, then in the simulations men veto players will veto the majority's choice with the likelihood observed in the experiments for men and women respectively.

E Experimental Design and Methodological Matters

E.1 Logistics and Procedures

The experiments were programmed using z-Tree (Fischbacher 2007) and conducted at a large U.S. university using z-Tree unleashed (Duch, Grossmann and Lauer 2020). Adult subjects ($>$ 18 years old) were drawn from a database with about 2600 registered subjects operated by a standard experimental social science laboratory at the large university in the United States. Subjects are registered undergraduate students. A computer routine used a random process to send out e-mail announcements, and sign-ups occurred as voluntary responses to those announcements.

After participants gave their consent to participate in a session, the experimenters produced the unleaded z-Tree links and sent the links to the registered participants. When all participants were connected to the server over the links, the experimenters started the session. Participants remotely joined the session over Zoom in which everyone's identity was purposely blinded. The experiment instructions were read out loud by the same experimenter. The experimenters shared their screen with participants while reading the instruc-

tions so that participants could also read the instructions by themselves. If subjects had questions in the middle of the experiment, they could send a private text message to the experimenters over Zoom chat. The experimenters then answered their questions instantly and privately. Participants were paid over a widely used online payment platform immediately after a session was concluded.

In Experiment I, 80 subjects, 48 men and 32 women, were randomly assigned to experimental conditions of **4M1W** and **2M3W**. Each session consisted of one group only. All these 80 subjects were regular group members without veto power. In Experiment II, another 80 subjects, 64 men and 16 women, were randomly assigned to experimental conditions of **MVeto-4M1W** and **WVeto-4M1W**. Among men, 56 served as regular group members, 8 served as veto players. Among women, eight served as regular group members and eight served as veto players. As in Experiment I, group members had no information regarding the gender composition of their group or the session. Only participants with veto power knew whether a veto was used; the regular group members received no feedback regarding how the others voted and were not told whether a veto was used. In Experiment III, another 60 subjects were recruited from the same subject pool used for Experiment I and Experiment II. They were randomly paired with decisions from previous simultaneous vote sessions. Within this group, 20% ($n = 12$) became regular group members, while 80% ($n = 48$) became veto players. All the 12 regular group members are women. Half of the veto players are women. These 60 participants were explicitly informed that they were matched with decisions from prior sessions, and no deceptive practices were employed. For these 60 participants, we applied the same random assignment procedure as in Experiment II, replicating identical variables such as information costs, the state of the world, and the voting choices of fellow group members. Consequently, these 60 participants made decisions under the same information and parameterizations as the 80 subjects in Experiment II.

We designed our experiments and controlled the decision-relevant information that participants had, so that the decision task and information structure is identical for subjects who participated in different sessions. We find no evidence showing session effects; the behavior of subjects who serve the same role is statistically identical across experiments (as shown in Table A2). Our empirical strategy enables us to maintain the rigorous controls of laboratory experiments while examining the robustness of our findings to alternative explanations such as strategic effects. Moreover, it helps us achieve high data collection efficiency in conducting group-based experimental studies.

E.2 Comprehension Quiz

After we read the experiment instructions and before subjects made decisions in the experiment, subjects were asked to complete a comprehension quiz. There was no time limit for the comprehension quiz. Subjects were allowed to ask questions. Only when one answered all the comprehension quiz correctly, and when all subjects completed the comprehension quiz, subjects could start making formal decisions.

1. The composition of your group changes from round to round. Yes or No? [Answer: No]
2. In each round, there are five participants in your group (you and four other participants). Yes or No? [Answer: Yes]
3. At the beginning of each round, the computer randomly determines which Jar, Jar 1 or Jar 2, is assigned to your group. Each Jar is equally likely to be chosen. Yes or No? [Answer: Yes]
4. Because each Jar will be chosen with a 50% chance, if Jar 1 was chosen for your group in the last three rounds, does it mean that Jar 2 will be chosen for your group in this round (with a somewhat higher probability)? Yes or No? [Answer: No]
5. Suppose you decide to acquire information, your willingness to pay for the information is less than the randomly generated information fee, do you receive the information? Yes or No? [Answer: No]
6. Suppose you decide to acquire information, your willingness to pay for the information is higher than the randomly generated information fee. You will receive the information and the amount you pay is the information fee but not your willingness to pay. Yes or No? [Answer: Yes]
7. If you acquire and receive the information, you will receive correct information about the Jar assigned to your group 90% of the time and incorrect information 10% of the time. Yes or No? [Answer: Yes]
8. Whether you acquire information or not, you can vote and your ballot affects your group's choice. Yes or No? [Answer: Yes]
9. If you voted for the right Jar but your group's decision is not the right Jar, your benefits will be the high payoff (i.e. 100 Points). Yes or No? [Answer: No]
10. If you voted for the wrong Jar but your group's decision is the right Jar, your benefits will be the low payoff (i.e. 0 Points). Yes or No? [Answer: No]

E.3 Experiment Instructions

Experiment instructions can be found at this link. We report the experiment instructions for the majority voting as a sample. The instructions for the other experimental conditions are similar and available upon request. The experiment consists of three parts. The instructions for Part 2 were distributed to participants after Part 1 has been finished. The instructions for Part 3 were announced at the completion of Part 2. In each session, everyone received the same instructions. In all sessions, the instructions were read by the same experimenter. There was no feedback of each part's results and earnings until the end of the session. Participants were allowed to ask question at any time. If subjects had questions in the middle of the experiment, they could send a private text message to the experimenters over Zoom chat. The experimenters then answered their questions instantly and privately.

E.4 Interface and Screenshots

The screenshots of the computer interface that subjects used in the experiments can be found at this link.

E.5 Pre-Registration

A pre-registration and pre-analysis plan for the study was filed with [name redacted]. The screenshots of the pre-registration can be found at this (identity redacted) link. The analysis choices reported in the main text and our research hypotheses were pre-specified in the preregistration. There are no departures from the pre-analysis plan.

E.6 Sample Size and Power Analysis

One of the most critical components of experimental research design is sample size estimation. A prevalent concern in controlled laboratory experiments is the statistical power of the findings, particularly when compared to larger-scale studies, such as surveys, where sample sizes tend to be more substantial. We recognize the importance of addressing these concerns in our sample size calculations and power analyses. A sufficient sample size is essential for reliably detecting true differences and ensuring that the study is not underpowered. Additionally, researchers must be cautious to avoid excessive sample sizes, which can increase the risk of Type I errors and lead to wasted resources. As a result, our experiment design includes careful estimation of the minimum required but sufficient sample size needed to achieve the desired power for detecting scientifically meaningful differences while maintaining a fixed Type I error rate.

Our experimental research design involves group-based voting experiments that feature fixed matching and a series of 20 voting periods with varying parameterizations, conducted without communication and with limited feedback. Participants are randomized into groups. All members within a group receive the same intervention (including the voting rule, gender composition, and the stochastically generated state of the world and information cost). To accurately account for the intra-group correlation—given that group performance across the 20 voting periods within the same group are likely to be correlated—we incorporate this consideration into both our sample size calculation and statistical analysis. Specifically, our sample size calculation involves two key tasks: (1) determining the number of groups required, and (2) establishing the number of participants needed within each group. The outcomes of the group voting are binary variables, while individual willingness to engage in costly information acquisition is measured using a continuous variable. To our knowledge, there is no available software package, built-in algorithm, or easy formula that directly addresses the power analysis behind our research designs. Thus, we draw upon existing theoretical and methodological discussions in the literature (Donner and Koval 1987; Morton and Williams 2010; List, Sadoff and Wagner 2011; Ahn, Heo and Zhang 2014; Chandar et al.

2019; Clifford, Sheagley and Piston 2021) to guide our sample size calculations, and we develop formal protocols for these calculations and power analysis, as detailed below.

E.6.1 How many groups do we need?

In each experiment, subjects are randomly assigned to one of two experimental conditions. For clarity in the power analysis, we refer to these as the *control group* (`c`) and *treatment group* (`t`). In both groups, there are $n_1 = n_2 = n_{groups}$ groups, each consisting of a fixed number of members, conducting group voting over $m = 20$ periods with varying parameterizations. In each period, groups vote between two alternatives, $w \in \{\mathbf{1}, \mathbf{2}\}$. There are two equally likely states of the world, $\omega \in \{\mathbf{1}, \mathbf{2}\}$, and group members receive a payoff of $\overline{u}$ if the adopted alternative $w = \omega$, and 0 otherwise. The goal of the group is to collectively choose the alternative that matches the state of the world.

Let Y_{ijk} be the binary random variable representing the jth ($j = 1, \ldots, m$) observation in the ith ($i = 1, \ldots, n$) group of the kth experimental condition ($k \in \{\mathtt{c}, \mathtt{t}\}$). Here, $Y_{ijk} = 1$ indicates that the group's choice matches the state of the world, while $Y_{ijk} = 0$ indicates otherwise. Following the general setup of existing theoretical and methodological discussions in the literature, we assume that observations within a group are exchangeable; given m, the observations $Y_{i1k}, \ldots, Y_{imk}$ share a common marginal probability $P(Y_{ijk} = 1) = p_k$ ($0.5 < p_k < 1$) and a common intra-group correlation coefficient, $\rho = \frac{\sigma_B^2}{\sigma_B^2 + \sigma_W^2}$, where σ_B^2 is the between-group variance, and σ_W^2 is the within-group variance. The correlation coefficient ρ reflects the clustering effect and the degree of similarity of observations both within and between groups.

Carefully addressing ρ is crucial for sample size calculations and power analysis in experimental designs like ours (Morton and Williams 2010; List, Sadoff and Wagner 2011; Fréchette 2012). On one hand, the play of a series of voting games with varying parameterizations offers several advantages. For instance, it reduces the likelihood of Type II errors caused by stochastic factors (Morton and Williams 2010), such as random guesses about the state of the world by uninformed participants and extremely high/low information cost stochastically generated in the experiments. Each period operates independently, with relevant parameters—such as the state of the world and information costs—redrawn from period to period. Communication is prohibited both within and between periods. Participants receive limited feedback on the previous period's group decision, but no information about realized costs, who acquired expertise, or the voting decisions of others. This limited feedback minimizes potential cross-decision influences (Fréchette 2012) while preserving the nature of group voting, where outcomes are collectively determined by the group rather than by an individual. On the other hand, treating each period's observation as independent can underestimate the true p-value and inflate the type I error rate, since correlations among observations within a group are typically positive (Blainey, Krzywinski and Altman 2014). Therefore, it is essential to employ statistical methods that account for the dependence of within-group observations when conducting sample size calculations and power analyses in

similar experimental designs.

We aim to test for differences in the quality of group decision-making, denoted as $P(Y = 1)$, between the control and treatment groups. We use one-tailed hypothesis testing for the calculations of sample size and power analysis. The null H_0 and alternative H_1 hypotheses are as follows:

> H_0: The quality of group decision-making is equal between the control and treatment groups, $p_{\mathtt{c}} \geq p_{\mathtt{t}}$.
>
> H_1: The quality of group decision-making differs between the control and treatment groups, $p_{\mathtt{c}} < p_{\mathtt{t}}$.

The quality of group decision-making in group k is estimated by

$$\hat{p}_k = \frac{\sum_{i=1}^{n} \sum_{j=1}^{m_{ik}} Y_{ijk}}{\sum_{i=1}^{n} m_{ik}}, \ k \in \{\mathtt{c}, \mathtt{t}\}.$$

Conditional on the empirical distribution of m_{ik} and the estimate of $\hat{\rho}$, the variance estimate of $\hat{p}_k$ is

$$V(\hat{p}_k) = \hat{p}_k(1 - \hat{p}_k) \frac{\sum_{i=1}^{n} m_{ik} \left[1 + (m_{ik} - 1)\hat{\rho}\right]}{\left(\sum_{i=1}^{n} m_{ik}\right)^2}, \ k \in \{\mathtt{c}, \mathtt{t}\}$$

Since $n_1 = n_2 = n_{cmte}$, the variance of $(\hat{p}_{\mathtt{c}} - \hat{p}_{\mathtt{t}})$ is estimated by

$$V(\hat{p}_{\mathtt{c}} - \hat{p}_{\mathtt{t}}) = \left[\hat{p}_{\mathtt{c}}(1 - \hat{p}_{\mathtt{c}}) + \hat{p}_{\mathtt{t}}(1 - \hat{p}_{\mathtt{t}})\right] \frac{1 + (m - 1)\hat{\rho}}{n \cdot m}.$$

We reject the null hypothesis $H_0 : p_{\mathtt{c}} = p_{\mathtt{t}}$ at the significance level of α if

$$\left| \frac{\hat{p}_1 - \hat{p}_2}{\sqrt{V(\hat{p}_1 - \hat{p}_2)}} \right| > z_{1-\alpha},$$

where $z_{1-\alpha}$ is the $100(1 - \alpha)$th percentile of the standard normal distribution. Under the alternative hypothesis, the power of the test is approximated by

$$\Phi \left(\frac{|p_{\mathtt{c}} - p_{\mathtt{t}}|}{\sqrt{\left[p_{\mathtt{c}}(1 - p_{\mathtt{c}}) + p_{\mathtt{t}}(1 - p_{\mathtt{t}})\right] \frac{1 + (m-1)\hat{\rho}}{n_{cmte} \cdot m}}} - z_{1-\alpha} \right).$$

The sample size estimate needed to achieve a power of $1-\beta$ can be obtained by solving the following equation:

$$\frac{|p_{\mathtt{c}} - p_{\mathtt{t}}|}{\sqrt{[p_{\mathtt{c}}(1-p_{\mathtt{c}}) + p_{\mathtt{t}}(1-p_{\mathtt{t}})]\,\frac{1+(m-1)\hat{\rho}}{n_{cmte}\cdot m}}} - z_{1-\alpha} = z_{1-\beta}.$$

As a result, with the significance level of α and a power of $1-\beta$, the sample size for comparing the quality of group decision-making has the form

$$n_{cmte} = \frac{p_{\mathtt{c}}(1-p_{\mathtt{c}}) + p_{\mathtt{t}}(1-p_{\mathtt{t}})}{(p_{\mathtt{c}} - p_{\mathtt{t}})^2} \cdot \frac{1+(m-1)\hat{\rho}}{m} \cdot (z_{1-\alpha} + z_{1-\beta})^2. \tag{E.1}$$

Different experimental designs lead to varying data generation processes, resulting in different estimates of $\hat{\rho}$. Based on our simulations, experiences, and our previous research, we anticipate significant variance driven by the varying parameterizations (e.g., the state of the world and information cost) across the 20 group voting periods, and thus we speculate that $\hat{\rho} = 0.1$ for this study. Based on our predictions and simulations, in a group with five members, when none of the group members are informed, $P(Y=1|N_5^{i=0}) = 0.5$; when one of them is informed, $P(Y=1|N_5^{i=1}) = 0.65$; when two of them are informed, $P(Y=1|N_5^{i=2}) = 0.8$; when three of them are informed, $P(Y=1|N_5^{i=3}) = 0.92$; when four of them are informed, $P(Y=1|N_5^{i=4}) = 0.97$; when all members are informed, $P(Y=1|N_5^{i=5}) = 0.99$. We speculate and use $p_{\mathtt{c}} = 0.65$ and $p_{\mathtt{t}} = 0.85$ to calculate the required sample size. To achieve 80% power at a 5% significance level with $m = 20$, the required number of groups for each intervention group is the smallest integer greater than or equal to n_{cmte} from Equation E.1:

$$n_{cmte} = \frac{[0.65(1-0.65) + 0.85(1-0.85)] \cdot [1+(20-1)\cdot 0.1] \cdot (1.645+0.842)^2}{(0.65-0.85)^2 \cdot 20} \approx 7.96.$$

Thus, in each experiment we need eight groups for the control group and eight for the treatment group. It is important to note that the statistical method for sample size estimation aligns with the statistical method for data analysis. We will use clustered parametric tests to examine the difference between $p_{\mathtt{c}}$ and $p_{\mathtt{t}}$. Additionally, we will double-check our findings by performing a more conservative, analogous non-parametric Fisher-Pitman permutation test, which relies on fewer and weaker assumptions and exhibits the highest power (100%) compared to related tests (Siegel 1957). A Monte Carlo study by Moir (1998) demonstrates that Fisher-Pitman permutation tests maintain statistically reliable power with as few as eight observations per treatment category.

E.6.2 How many subjects do we need?

To understand the factors influencing differences in group performance, we also aim to explore differences between women and men in terms of their preferences for cooperation and risk. We conduct a threshold public good game, which is widely applied to elicit social preferences, and we use individuals' contributions as a proxy to examine whether women are

more collaborative (Palfrey and Rosenthal 1984; Bicchieri 2016). In this game, each member of the same 5-person group decides how much of 100 experimental currencies (ECUs) to contribute to the group account. Only when the group's aggregate contribution is equal to or over the threshold (i.e., 250 ECUs), individuals can benefit from the contribution. Otherwise, if the group's aggregate contribution is less than the threshold, everyone loses their contribution. We measure risk preference using the method developed by Gneezy and Potters (1997). Each decision-maker chooses how much of 100 ECUs she or he wishes to invest in a risky option and how much to keep. Both of these two measurements are incentivized with real monetary consequences. These measurements are incentive-compatible such that participants can achieve the best outcome by acting according to their true preferences.

The preferences for cooperation and risk are elicited separately from the collective voting process, ensuring that the experimental conditions and interventions do not influence the measurement of these preferences. Following the work of Bicchieri (2016); Gneezy and Potters (1997) and related findings documented in the literature, we assume $|\bar{y}_1 - \bar{y}_2| = 15$ and $\sigma_1^2 = \sigma_2^2 = 20$. To achieve 80% power at a 5% significance level, the required sample size is $n = \frac{(\sigma_1^2+\sigma_2^2)(z_{1-\alpha}+z_{1-\beta})^2}{(\bar{y}_1-\bar{y}_2)^2}$. This implies that we need at least 22 women and 22 men to detect the predicted gender differences in preferences for cooperation and risk.

In the experiments, we treat information acquisition as a unified process throughout the group proceedings, and we measure individuals' willingness to exert costly effort to obtain information. Specifically, before casting their votes, each group member independently decides how much effort to invest in acquiring information for themselves. Each participant is given 100 ECUs, which they can either keep for exchange into U.S. dollars or spend on acquiring information about the true state of the world. An individual's *willingness to acquire information* is measured by the amount they are willing to pay for this information. The difficulty of the problem is measured by the *ex-ante* unknown cost of information, which is randomly generated by the computer as an integer between 1 and 100. Participants are informed of the true state of the world and paid for the stochastically generated information cost when their willingness to acquire information meets or exceeds the cost. Since the cost of information is randomly generated, the more effort a participant exerts, the more likely they are to be informed.

While gender differences in preferences for cooperation and risk are well documented in the literature, we find little empirical studies that offer clear expectations regarding the differences in means and variances of gender differences in information acquisition. However, since we use the same 0-100 scale to measure effort in information acquisition, and given the context of group decision-making, we expect correlations between individuals' information acquisition decisions and their preferences for cooperation and risk. Based on our experience and simulation analysis, we speculate that the differences in means and variances of information acquisition are similar to those observed for cooperation and risk preferences.

Let n_1 be the sample size estimate from a simple randomization trial without clustering effects. We assume $|\bar{y}_1 - \bar{y}_2| = 15$ and $\sigma_1^2 = \sigma_2^2 = 20$. To achieve 80% power at a 5% signifi-

cance level, the required sample size is $n_1 = \frac{(\sigma_1^2+\sigma_2^2)(z_{1-\alpha}+z_{1-\beta})^2}{(\bar{y}_1-\bar{y}_2)^2} \approx 22$. In our experiments, the gender composition is unknown to the participants. Since we construct anonymous experimental environments and control the interactions and feedback within groups, the sample size calculations and power analysis only need to account for the aggregate clustering effects on individuals' information acquisition decisions. We address the clustering effects by multiplying n_1 by the variance inflation factor $1 + (\zeta - 1)\hat{\rho}$, where $\zeta = 5$ represents the constant group size across all groups and $\hat{\rho} = 0.1$ is the intracluster correlation coefficient that reflects the clustering effect and the degree of similarity of observations both within and between groups. This gives $n_1(1 + (\zeta - 1)\hat{\rho}) \approx 31$. Since we have eight groups for each experimental condition, and to maintain symmetry, we require at least 32 women and 32 men to detect the predicted gender differences in information acquisition. Based on these analysis, the gender compositions that include both women and men, and vary as women-majority and men-majority groups, are fixed. These analyses also apply when calculating the sample size and performing power analysis for experimental designs that incorporate veto players.

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